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THE MEASUREMENT OF "SPONTANEOUS ACTIVITY"*

From the Laboratory of General Physiology, Harvard University

B. F. SKINNER[1]

I

In the traditional use of the term "spontaneous activity" no attempt is made to distinguish between the various forms of the behavior of the organism. Taken in this broad sense, a quantitative measure of the exhibited activity is practically impossible. An indirect measure may be obtained from some aspect of the metabolism of the organism, its heat production, for example, or its oxygen consumption. But a direct measure of behavior, as such, must provide for each of the many forms the behavior may assume and is consequently very difficult. If we suppose, in spite of the technical difficulties involved, that it is nevertheless possible, and that a separate measure of each form of behavior can be made available, we have still to provide for the reduction of the resulting data to a common unit expressing "activity."

The use of a "running-wheel" supplies a practical solution. What it does, in effect, is to select one kind of activity for measurement and so far as possible to suppress all other kinds. It utilizes for the purpose a form of behavior easily measured, with a result expressible in simple units. Thus, if a rat is confined in a small cage with access to a running-wheel, its "activity" during any given period of time can be read from a counter on the wheel simply as distance run. This is a convenient procedure and has been widely used.

It is obvious, however, that the method involves several broad assumptions. The behavior of the rat in the wheel will depend, first of all, upon the properties of the wheel. It will depend also upon the degree of suppression of other kinds of behavior and upon the periodic appearance of those kinds which cannot be suppressed if

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[1] National Research Council Fellow.

any considerable interval of time is being dealt with. It will depend also upon certain incidental changes in the environment, which are often not eliminated, as, for example, in illumination, sound, and temperature. In an experiment concerned chiefly with the internal comparison of a group of total distances run, measured under the same conditions, these factors are not necessarily important. But in any exact analysis of the behavior of the rat they must be taken into account.

It is the purpose of the present paper to examine some of the variables of which behavior in a running-wheel may be a function. A standard wheel, a method of recording, and an experimental procedure are described. Sample records are given to illustrate the application of the method to several problems concerning spontaneous activity. The nature of the general phenomenon is also briefly discussed.

II

In considering such a system as a rat in a running-wheel it will be convenient to make several simplifying assumptions. We shall not need to examine the specific postural and progressive reflexes evoked as the rat runs, or their interaction. We shall assume that only a single force is exerted upon the wheel and that this acts tangentially. We shall assume that the running is always in a plane normal to the axis of the wheel, and that, when the rat starts running, a given velocity (V) *relative to the running-surface* is developed during a very brief period of time (which can be ignored) and is then constantly maintained until the rat stops. We shall also assume for the moment that the wheel is frictionless. The behavior of such a system will depend almost wholly upon the ratio of the weight of the rat to the moment of inertia of the wheel, expressing the latter in grams at the radius of the running-surface. We may consider three cases.

a. Where the moment of inertia is negligible, the absolute position of the rat is not changed, and the velocity (V) is assumed entirely by the running-surface.

b. As an intermediate case, suppose the weight of the rat and the moment of inertia to be approximately equal. The resting rat stands at a position of equilibrium with its center of gravity directly above the lowest point in the perimeter of the wheel. The acceleration at the start is divided equally between wheel and rat. With the acquired absolute velocity of $V/2$ the rat moves out of the position of equilibrium at A (Figure 1) toward a position as at B. This introduces a new force tending to accelerate the wheel, namely, the gravitational component, equal to the product of the weight of the rat into the sine of the angle of displacement (a' in the construction in Figure 1). Since we have assumed a constant relative velocity, the movement toward B continues until the absolute velocity of the wheel has become equal to V. But the rat is then in a position as at B. Consequently the wheel is further accelerated, and the velocity of the rat becomes negative. The rat passes through position A, having at that point a negative velocity of $V/2$, toward a position as at C, where the gravitational component brings the velocity of the rat again to zero and of the surface again to V. The gravitational component then further reduces the velocity of the wheel, giving the rat a positive velocity which reaches $V/2$ at A, completing the cycle. If we hold strictly to our assumptions of a constant relative velocity and a frictionless wheel, the rat will oscillate in this fashion about A, continuing meanwhile to run with the constant relative velocity V. The wheel and the running rat may be treated simply as a pendulum, the laws of which apply to the present system.

When the rat stops, the relative velocity is reduced to zero, and the rat is carried toward C until the gravitational component reduces

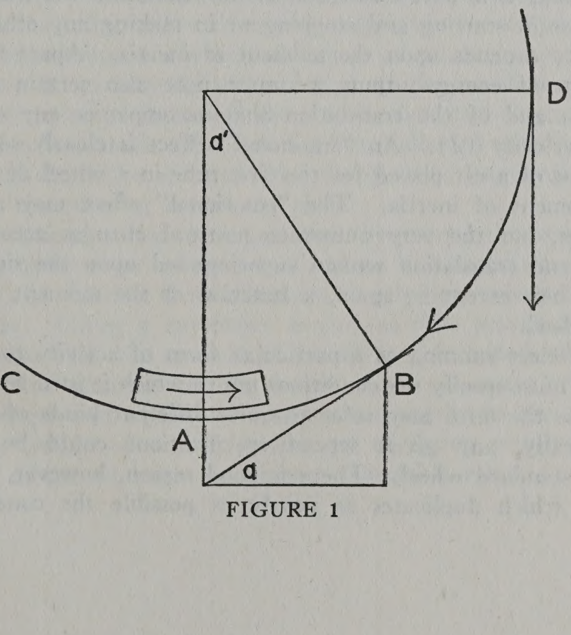

FIGURE 1

the absolute velocity to zero. From this point the wheel and the now resting rat again behave as a pendulum.

c. In the third case, where the moment of inertia is greater than the weight of the rat, case *b* holds, but the movement of the rat from the position *A* is proportionately greater in both directions.

The effect of friction, which we have thus far ignored, is, first, to damp any oscillation about *A*, and, second, to determine the point at which equilibrium is reached by the running rat. When oscillation has been eliminated, the rat runs at a position such that the gravitational component is just equal to the friction of the wheel, the latter being expressed in grams acting vertically at the radius of the running-surface (*D* in Figure 1). This relationship will be considered again later.

A constant relative velocity cannot actually be expected. The change in slope resulting from any movement out of the *A* position usually reduces the relative velocity on the *B* side and increases it on the *C*. The effect of the change is to reduce the extent of any oscillation. There may also be changes in velocity not related to the system, the effect of which is either to amplify or to reduce an oscillation.

It is obvious that the behavior of the rat will vary with these properties of the wheel. The energy expended in running at a constant velocity is in part a function of the friction of the wheel. The work done in starting and stopping or in making any other change in velocity depends upon the moment of inertia. Apart from considerations of energy-output, we must note also certain effects of oscillation and of the translation that accompanies any change in relative velocity (2). An "emotional" effect is clearly seen in the excitement of a rat placed for the first time in a wheel of relatively great moment of inertia. The "emotional" effect may adapt out with time, but the very numerous postural changes accompanying rotation and translation remain superimposed upon the simple running. Their extent is, again, a function of the moment of inertia of the wheel.

If we select running as a particular form of activity to be measured, we must specify the conditions under which it is to be observed. Otherwise the term may refer to quite different kinds of behavior. Theoretically, any given set of specifications could be used to define a standard wheel. There is good reason, however, to prefer that set which duplicates as nearly as possible the conditions of running on a level surface. This is not only the most natural form of the behavior (the most often observed under ordinary conditions), and consequently the least disturbing, but also mechanically the simplest. The running-wheel may, in short, be regarded as a substitute for a level straightaway, which would be the ideal instrument for the measurement of running if it were not obviously impractical; and the wheel should be constructed accordingly.

The characteristics of a level straightaway cannot, of course, be exactly reproduced in a wheel. In order to duplicate the work expended in any acceleration, the wheel must have a moment of inertia equal to the weight of the rat. But this introduces at once the rotational and translational effects we have already noted, which are lacking in straightaway running. Beside this inherent difficulty there are also technical limitations to be considered in the construction of the wheel which arise chiefly from a third factor which we have not yet considered, namely, curvature.

If we are to reproduce as nearly as possible the characteristics of a level straightaway, the greatest section of the perimeter of the wheel touched at any instant by the rat must be sensibly flat. This would not be a difficult requirement if there were no limit to the available radius. Unfortunately, however, the moment of inertia of the wheel increases rapidly with the radius. For this reason our present problem is virtually to determine the greatest curvature (the smallest radius) that will still remain negligible in comparing the mechanics of running in a wheel with those of running on a flat surface. There is no very definite solution of the problem at hand. In the construction of the wheel about to be described a considerable allowance has been made: The rule has been followed that the radius of the wheel should be at least twice the greatest length of surface touched by the running rat at any time.

Since an optimal curvature remains for the present almost a matter of opinion, there is no unique set of values for curvature, moment of inertia, and friction that will most closely approximate a level straightaway. Given a curvature as low as that provided by the half-the-radius rule, the moment of inertia is determined wholly by structural considerations, for the lowest value consonant with reasonable strength and rigidity will almost necessarily be greater than the average weight of a rat. A moment of inertia equal to twice the weight of the rat might be set as a reasonable value.

Such a wheel can be greatly improved (so far as our standard of reference is concerned) by the addition of a small amount of friction. The friction damps the oscillation and thus to some extent corrects for the undesirable effect of the necessary weight of the wheel. Assuming the weight of the rat to be 175 grams and the moment of inertia 350 grams, a sliding friction of 10 grams (expressed as above) will successfully damp any ordinary oscillation within one or two cycles. Against such a friction the rat runs at a slope of a little over 3°, which is not a very significant deviation from a level path.

Certain other properties of a suitable wheel may be considered.

1. In order to insure a correct linear measure of distance run the running must take place, as we have assumed, in a single plane normal to the axis of the wheel. The pathway must consequently be wide enough to permit unobstructed running but narrow enough to prevent wandering from side to side.

2. The character of the surface must be adapted to level running but must also provide an adequate grip, both for climbing a considerable slope and for controlling the momentum of the wheel during any change in velocity.

3. The principal strain on the wheel is centrifugal. The construction must allow for the support of the weight of the rat and for the centrifugal forces present in running. A further strain, moreover, must be provided for. Direct measurements of frequency of stepping, obtained by recording the pressure exerted upon the substratum by a running rat, show that each foot may strike the surface seven or eight times per second, giving a total frequency of 28 to 32 impacts per second. This is for fairly brisk running, but a considerably higher value may well be possible. A running-wheel of the size and strength about to be described may have a natural frequency within this range. The wheel must, therefore, be constructed to stand the extra strain imposed upon it when that frequency is reached and to avoid so far as possible any extra vibration, which would be a significant source of disturbance.

4. If friction is applied to the shaft, very little torsion of the wheel about its axis should be possible. Otherwise the wheel will "whip" during any change in velocity.

5. The wheel should be quiet.

III

A wheel fulfilling these requirements may be constructed as follows:

A piece of drill rod (*C* in Figure 2), 10″ x 5/16″, is mounted horizontally on sets of ball bearings (*E,E*), which are pressed into holes in two (2″ x 4″) wooden uprights (*F,F*). The shaft carries two collars (*B,B*), each of which has six equally spaced holes in its rim, countersunk on alternate sides to receive standard bicycle spokes. The rims are 2 1/4″ apart. Two aluminum tubes (cross-section shown at *A,A*), 6 feet long and with an outside diameter of 1/4″, are bent into hoops with a tinsmith's bender. Six equally spaced holes are drilled radially into each hoop with a No. 38 drill. One hole comes at the juncture of the two ends and is not drilled but filed, one-half in each end, with a small round file. Bicycle spokes, 11 1/2″ long, are inserted into the collars and then into the hoops, one collar being connected with one hoop only. The spokes are crossed in pairs in the usual manner. Number 3-48 nuts are screwed on the spokes outside the hoops, and tightened until the latter are firmly held. The ends of the hoops are pressed together in the process, and no other fastening is required.

At this stage the wheel consists of two aluminum hoops mounted separately upon a shaft. A strip of aluminum mesh, eight strands to

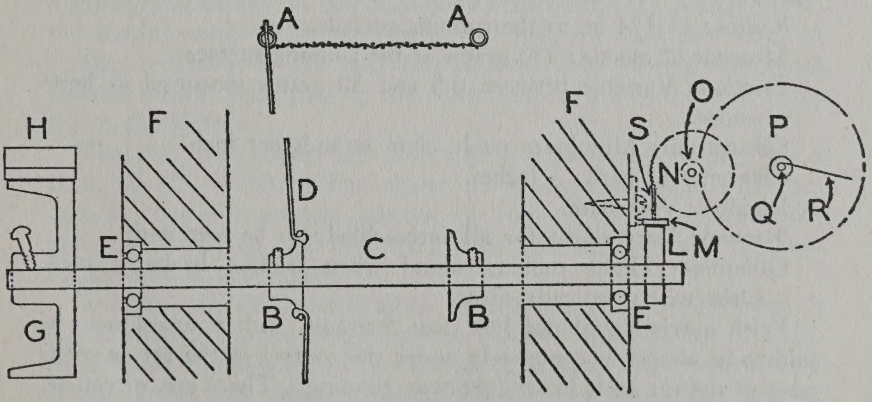

FIGURE 2

Schematic Section through the Axis of the Wheel, Showing Friction-Brake, Recording Device, etc.

For details see text

the inch, 3 1/4″ wide and 6 feet long (the edges being suitably hemmed) is then inserted between the two hoops. The screen rests on the inner surface of the hoops, the hem being indented at the proper places to accommodate the spokes. The screen is wired to the hoops with short pieces of aluminum wire, and the two ends of the strip are woven together.

The width of the screen is greater than the distance between the rims of the collars. The spokes consequently flare slightly outward and provide a lateral pull which is of service in truing the wheel. This is done by manipulating the tension on each spoke. A lateral variation of less than 1/4″ can easily be obtained.

In order to provide a variable friction (for purposes to be mentioned later) an aluminum drum (*G*), 3″ in diameter, is attached to one end of the shaft. A single layer of copper or iron wire is carefully wound about the drum to provide the friction surface. A leather brake (*H*) is arranged to be applied with any desired force by a weight and lever of variable length.

The wheel is left open on both sides. If it is mounted at least 2 feet above the nearest surface, a rat will not, under ordinary circumstances, escape. Guard plates may be mounted on the uprights if desired. A living-cage is also easily attached to either upright with a door leading into the wheel.

The significant characteristics of the wheel may be listed as follows:

Radius: 11 1/4 in. at the running-surface.
Moment of inertia: 350 grams at the running-surface.
Friction: Variable between 0.5 and 50 grams measured as heretofore.
Substratum: Aluminum mesh, eight strands per inch.
Clearance of path: 3 inches.
Rigidity: Adequate.
Torsion: Very slight for all forces likely to be met with.
Quietness: Slight rubbing sound when friction brake is used. Otherwise practically silent.

With a friction of not less than 5 grams, such a wheel may be said to be almost as completely under the control of the rat as is the mass of the rat itself in straightaway running. There are, of course, slight mechanical differences, but when the initial adaptation to the new stimulation has taken place, the two cases can for most purposes be considered identical.

IV

The usual measurement taken with the running-wheel is the total distance run during some such period of time as a day, where the distance is expressed either in a unit of length or in revolutions of the wheel. Although such a datum has the dimensions of a velocity, it has no close relation to the actual running, since the latter is not continuous. A closer correspondence may be secured by reducing the length of the experimental period. The best representation is a continuous distance-time graph, which can be obtained experimentally with the following device:

An eccentric (*L* in Figure 2), attached to the shaft of the wheel, produces a reciprocal motion in the lever (*M*), which rests upon it. A tongue (*S*) fastened to the lever engages in the teeth of the ratchet (*N*) and turns the latter one notch forward at each revolution of the wheel, regardless of the direction in which the wheel is turned. The movement of the ratchet is reduced ten times by the gears *O* and *P*. If the ratchet has 48 teeth, the shaft (*Q*) turns once for each 480 revolutions of the running-wheel or for every 2880 feet run by the rat. A continuation of the shaft carries a silk thread (*R*) which is slowly wound up as the shaft is turned. The thread is led 2 or 3 feet from the device, at right angles to the shaft, passed over a glass ring, and fastened to a collar, which slides vertically upon a square shaft. A writing-point mounted upon the collar writes upon a kymograph drum. If the diameter of the shaft (*Q*) is about 1/6″, the writing-point will move vertically 1 inch for each mile run. A further reduction in this ratio can be secured by looping the thread about a second glass ring on the collar and fastening the end to the top of the shaft.

The resulting graphs are most easily read if the tracing has a slope of 40° to 60°. The choice of kymograph speed will thus depend upon the average amount of running to be recorded per experimental period and will vary with the rats used and the conditions of the experiment.

The scale of the records given by the apparatus as described is not adapted to the finer details of the running behavior, for the relatively small coordinates will not permit reading to a single revolution of the wheel. In order to obtain a greater excursion the ratio of the gears may be changed and the shaft enlarged. The gears may even be omitted altogether and the shaft of the ratchet used to wind the thread. An excursion of 1 inch per ten revolutions is easily obtained. A relatively fast kymograph must be used to give the records a slope at which their characteristics are clearly indicated.

In the case of large amplifications of the vertical movement of the writing-point an impractical width of kymograph paper is required. The *effective* width of the usual 6″ paper may be increased ten- or twenty-fold with the device represented in Figure 3. The spool (*A*), mounted on the sliding carriage of the writing-point (*B*), carries a supply of extra thread. The spool is held from unwinding by the arm (*C*) and the catch (*D*). The entire device is supported by the thread (*F*) which goes to the running-wheel. As the carriage is drawn up, a string (*E*), fastened at one end to the lever (*D*) and at the other to the base of the stand, is drawn tight. The string is of such a length that, when the writing-point reaches the top of the drum, the lever (*D*) has been lowered to a point at which the arm (*C*) is released. The spool (*A*) immediately unwinds. The whole device drops, and the lever (*D*) catches the arm (*C*) after only one revolution of the spool. A length of thread equal to the circumference of *A* is thereby introduced into the system. If the circumference is slightly less than the width of the kymograph paper the writing-point is almost instantaneously shifted from the top to

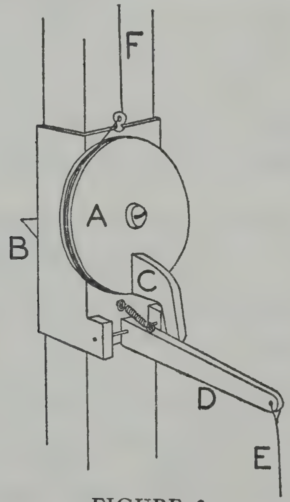

FIGURE 3
Device to Increase Effective Width of Kymograph Paper in Records of the Type Here Described

the bottom of the drum. The operation is repeated when the writing-point again reaches the top of the paper. The completed records must be cut vertically along the lines made by the falling point and pieced together to form a continuous graph. If the device is very light and the bearings of the lever (*D*) practically frictionless, no sensible distortion is introduced by the method.

V

It is a matter of common observation that such an organism as a rat is not continuously active. A distinction can be drawn between two general phases in its behavior, an active and a quiescent. If observed for a sufficient time the two phases recur alternately, and the behavior shows a periodicity. We are not concerned here with periodicity *per se* or with the mechanism of initiation of either phase. The reader may be referred to the papers of Richter (3) and Stier (7). We are concerned rather with the measurement of the active phase once initiated and the relation of the elicited behavior to certain independent variables. The periodicity is important, however, in its technical bearing upon the temporal conditions of an experiment.

Since the recording devices are entirely automatic, the complete apparatus may be set up under the desired conditions and the records of the behavior taken mechanically whenever the behavior is forthcoming. The rate of running can be measured in this way during a single active phase or during a series of phases. For certain purposes, however, it is convenient to make use of the fact that activity can be stored, and that when so stored the time of its elicitation can be controlled.

Suppose the behavior of a rat to be observed for successive periods of 24 hours each. The records are found to be composed of active and quiescent phases, with a possible relation between the distribution of the activity and the time at which daily care is given to the apparatus and rat. If we now introduce into the 24-hour period an interval during which access to the wheel is cut off, the remaining part of the period shows a greater activity per unit time, and the distribution is clearly related to the interval of confinement, in that the latter is followed by a relatively intense burst of activity. If the confinement is then gradually prolonged, the "density" of the activity during the rest of the period increases, until a length of interval is reached at which the remaining active period shows no quiescent phase. The length of such a period varies with many factors but may be of the order of 4 to 6 hours.

If there is no significant diurnal change in the environment, a rat quickly adapts to running at any given hour. Consequently, an experiment can be carried out whenever the conditions of the laboratory are most suitable, especially with respect to noise. The release of the rat is easily accomplished with an ordinary alarm clock, in the following way. A short extension is attached to the alarm key, which winds up a piece of string fastened to it and draws a latch-bolt. The unlatched door is opened by a spring or counterbalance. The alarm starts the kymograph in the same way. (In order to set an alarm more than 11 hours in advance it is necessary to combine two of the usual clocks. The second clock turns on the alarm of the first after the first has passed the hour at which it would otherwise have gone off. Two clocks permit a setting slightly less than 22 hours in advance.) It has been found most convenient for the experimenter to terminate the experiment in person, although this could also be done automatically.

The following procedure has been satisfactory. The experimental period ends at 8:30 A.M. At that time the record is removed from the drum and any necessary care is given the rat. The cage is then closed, a fresh drum is placed on the kymograph, and the clock set for the release. The length of active period is determined by the time of the release. For example, the alarm is set for 3:30 A.M. if a five-hour period is desired. No other attention is required during the 24 hours.

VI

Figures 4 and 5 are reproduced from a series of records obtained with a single female rat aged 125 to 175 days. They are supported, so far as the general comment made upon them is concerned, by several hundred records from many other rats, representing several thousand experimental hours. Most of these records were obtained while the design of the wheel was being evolved through a series of trial models. More extensive observations from a single model (as described herein) are in progress. The accompanying figures are given chiefly to show the sort of result to be expected and the applicability of the method to several problems concerning activity.

The records were obtained with the wheel and procedure already described. The rat was released into the running-wheel at 3:00 A.M.

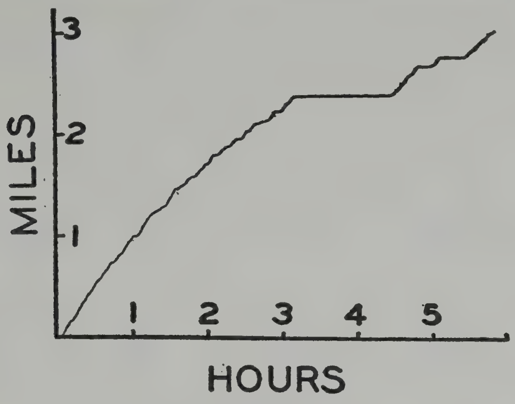

FIGURE 4

RECORD OF THE RUNNING ACTIVITY OF A FEMALE RAT DURING A ("COMPRESSED") PERIOD OF ABOUT SIX HOURS

The rate of running, given by the slope of the curve, declines in an orderly manner during the first three hours. Thereafter a greater variability is evident, but the extrapolation of the first part of the curve is nearly reached before the period ends.

and the experiments were terminated between 8:00 and 9:00 A.M. The usual precautions were observed. The temperature of the room varied less than 2°C. during the entire series of experiments and probably less than 0.5°C. during a single period. The average temperature was about 21°C. The room was dark during the running and during most of the period of confinement and was thoroughly sound-proofed. No other animals were present. Drinking water was always available.

Figure 4 is a typical daily record obtained with a friction of about 15 grams. The rate of running begins at a maximum.[2] During the next three hours the rate shows a gradual but remarkably smooth decline, which is best seen in Figure 4 by foreshortening the record. The variability of this part of the curve appears from inspection to be some function of the slope, increasing as the slope falls off. After three hours the record suddenly shows a greater variability (discussed below), but the extrapolation of the first part of the curve is later fairly closely approximated.

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[2] Some rats invariably "warm up" for 15 or 30 minutes before reaching a maximal rate.

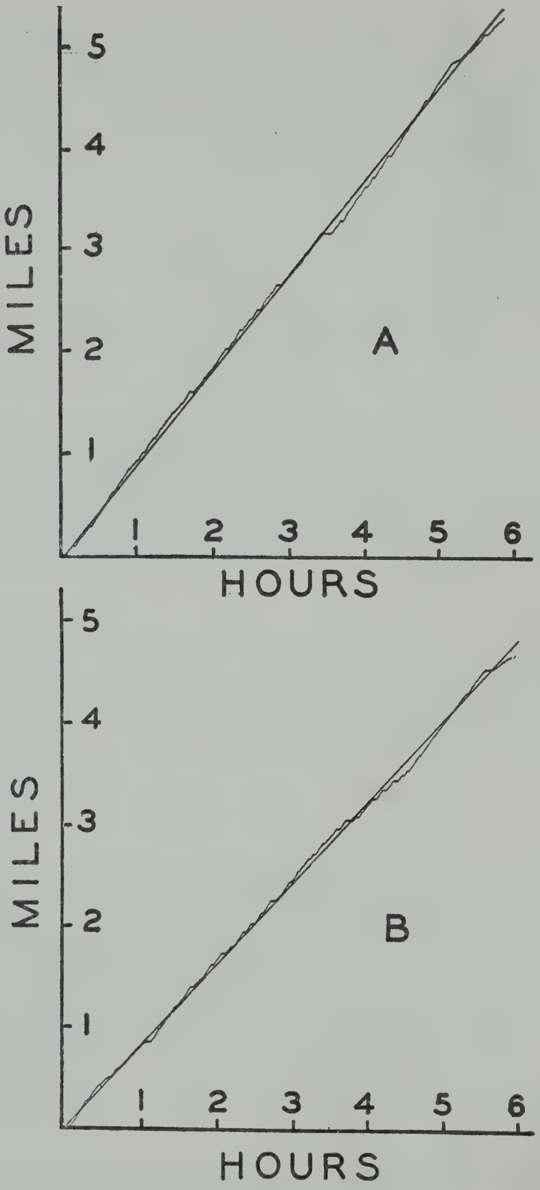

FIGURE 5

Records of the Running Activity of the Rat in Figure 4 Where a Conditioned Hunger Cycle Has Been Built Up with Its Peak at the Sixth Hour

The two records were taken on successive days. In *A* the friction of the wheel (expressed as described herein) was 14.3 grams. The slope of the straight line drawn through the curve is 52°. In *B* the friction was increased to 19.5, and the slope falls to 48°.

In order to obtain a record of this sort it is necessary to suppress the feeding behavior that would ordinarily appear during the period. This is done by attaching a cover to the food tray, which is closed at the beginning of the period by the release alarm. If this precaution is not taken, the curve is interrupted by delays for which there is no adequate compensation, and the form of the curve as revealed in Figure 4 is obscured. No very considerable hunger is developed during a six-hour period, although feeding behavior is undoubtedly suppressed.

If this procedure is repeated for several days, a conditioned hunger cycle is developed, the peak of which comes at the end of the experiment, when the rat is fed. After the rat has been fully conditioned to eat at, say, the sixth hour after the release, the form of the record changes to that shown in Figures 5*A* and 5*B*. A well-marked condition of hunger comes to be developed during the latter part of the period, which is reflected in an increase in activity. The resultant curve is essentially a straight line, if the removal of the food coincides with the beginning of the period of activity. The difference between the two types of curve is an indication of the way in which the conditioned hunger cycle is built up during the period, and, granted the possibility of a quantitative description, is thus a valuable measure of that process.

As we have seen, the rat runs in the wheel at a position such that the gravitational component acting tangentially is just equal to the friction. Given the weight of the rat (W) and the friction of the wheel (F) measured as above in grams acting vertically at the running-surface (at D in Figure 1), the position can be calculated from the simple relation

$$\frac{F}{W} = \sin \alpha$$

which is readily obtained from the construction in Figure 1. The apparatus, as described, includes a variable friction ranging from practically zero to about 50 grams. Beyond 50 grams technical difficulties are likely to arise in maintaining a constant coefficient of friction and in avoiding chattering or squeaking of the brake. The available range of slopes at which a 175-gram rat can be made to run with this wheel is, therefore, from practically zero to about 16°.

Figure 5 demonstrates also the effect of friction upon the behavior of the rat. The record *A* was obtained when the friction of the wheel was approximately 14.3 grams. Record *B,* taken the following day, is for a friction of 19.5 grams. The straight lines drawn through the records have the slopes 52° and 48° respectively. The total record is thus modified by the change in slope, and to the same degree throughout. The exact relationship between slope and rate is under investigation.

VII

The "normal" curve representing the compressed daily activity of the rat is obtained by limiting the time during which running is possible. The compression is carried out until a length of period is reached in which no quiescent phase occurs. This is an uncertain criterion, however, since even the most active running is broken by short "quiescent" pauses. Fortunately, the nature of the resulting curve enables us to define the length of a period of complete compression without appeal to the active-quiescent distinction.

The variability of the record in Figure 4 can be estimated (for the present simply by inspection) from its deviations from a smooth curve. These are due chiefly to the appearance of short periods of no running. For the first three hours in the record in Figure 4 the variability is, as we have seen, to a certain extent related inversely to the slope of the curve, increasing as the slope falls off. At the end of the third hour there is a sudden break, and no running is observed for more than an hour. We may deal with this delay, however, not as a quiescent phase, but as a *sudden increase in variability.* The process of compression is directed toward the elimination of such a change and may be defined as complete, therefore, when the behavior during the active period shows only the orderly change in variability related to the slope of the curve.

The active period represented in Figure 4 must, then, be regarded as too long. This does not mean that the period should have ended at the third hour. The effect of compression is not felt, of course, on the day it is made. According to the technique already outlined the length of period should have been shortened progressively until successive daily records showed no sudden change in variability. A length of four and one-half or five hours might have sufficed.

An accurate determination of the length of a compressed period is important for the quantitative treatment of the behavior. It will be observed in Figure 4 (and is a general characteristic of records of this type) that after an exceptional delay there is a compensatory increase in the rate of running, but the extrapolation of the first part of the curve is never quite reached. An equation describing the earlier part of the curve need not account adequately for the latter. Nevertheless it would be embarrassing to set an arbitrary limit to the datum to be described based wholly upon the ease of describing it. It should be noted, therefore, that the proposed definition of a compressed period is not based upon the simplicity of the resulting curve, although it has the effect of achieving it. The appeal to variability secures a limit not related to the nature of the curve. While the argument, as presented, is internal to the data, it is supported by the reasonable assumption that the sudden change in variability indicates the operation of a factor extraneous to the group previously controlling the behavior.

VIII

The word "spontaneous" was defended by Bethe (1) as properly designating a response for which the correlated stimulus was unknown. This is clearly the accepted meaning in the term "spontaneous activity." The unknown stimulus is in this case regarded as interoceptive, and many investigations of the subject have been concerned with its identification. The well-known stomach-contraction hypothesis, which, as Stier (7) has shown, was first suggested by Loeb, will serve as an example of this point of view.

For the sake of a consistent systematic use of the term stimulus, however, it should be pointed out that the stimuli that elicit running are known. The reflex mechanisms concerned with simple stepping and coordinated progression have been extensively studied. The monograph by Magnus (2) summarizes the literature up to the date of its publication. The specific stimuli are partly proprioceptive (depending largely upon the position of the limbs and the forces tending to change that position) and partly exteroceptive—visual, tactual, or of some other form. The supposed unknown stimulus implied in the use of the term spontaneous must act, not to elicit progression, but only to *facilitate* its elicitation by these proprioceptive and exteroceptive stimuli. This is an important distinction.

That an external stimulus may facilitate reflexes of progression is, of course, well known. Magnus, for example, has described the effect of a loud sound upon the progression of a thalamic rabbit (2, pp. 203-204). It is quite possible that an internal stimulus may act in a comparable fashion to provoke the phenomenon of spontaneous activity. However, our inferences concerning the nature of an internal factor are based upon certain observed aspects of behavior. Superficially, the outstanding characteristic of spontaneous activity is the suddenness of the transition from rest to action, and this doubtless has led to the current assumption of a *stimulus* as responsible for the change. As our description of spontaneous activity becomes further elaborated, however, the properties ascribed to an internal factor must be modified accordingly. Figure 4 suggests that the immediately antecedent central condition is not a stimulus, but a slowly changing state, the properties of which may be rather accurately inferred.

In any event the hypothesis of an internal stimulus (or state) is unnecessary in the study of spontaneous activity. Our immediate experimental material is the behavior of the rat. We start from the observation that, although a running-wheel as a source of stimulation may be constantly present, the reflexes of which running is composed are not always elicited or if elicited they vary in strength.[3] The problem of spontaneous activity arises in this apparent variability. In solving it we need not go beyond a quantitative description of strength as a function of some such independent variable as time. The identification of a "physiological correlate," which varies in the same way, may yield supplementary information but is not essential to the solution, so far as the description of behavior is concerned.

A similar argument has been advanced in the case of hunger (5, 6). In several respects a close parallel can be drawn between these two "drives." The normal eating curve is obtained with a technique of compression similar to that described herein, and the properties of the resulting curves are obviously alike. The record in Figure 4, for example, may be compared with Figure 6, which is reproduced from an earlier paper (5) on the eating behavior of the rat. The two curves are not quantitatively identical but their great similarity is obvious. Activity, however, presents a special problem. As we have noted elsewhere (5), a condition of hunger manifests itself as spontaneous activity whenever specific stimulation leading to a characteristic response is absent. Activity *per se* must therefore be regarded as subject to external complications of this sort. The interaction of two "drives" is demonstrated in Figure 5. A comparison with Figure 4 also suggests, however, the possibility of an independent experimental manipulation of each factor. A complete analy-

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[3] For this use of the term strength *cf.* (4) and (5).

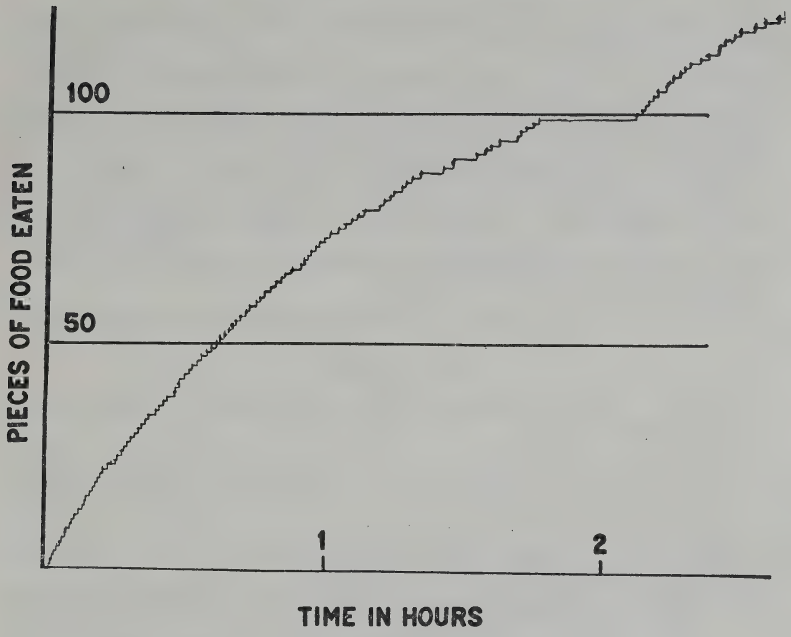

FIGURE 6

Record of the Eating Behavior of a Rat, Reproduced from an Earlier Paper (5) for Comparison with Figure 4

sis of the phenomenon should be possible with the procedure here described.

Summary

The "running-wheel" measures a selected form of spontaneous activity. The behavior is dependent in part upon the properties of the wheel. The mechanics of running in a wheel are discussed. In listing the specifications of a standard wheel an effort is made to reproduce as nearly as possible the conditions of running on a level straightaway. A suitable wheel is described. The method of recording yields a continuous distance-time graph of the behavior.

The behavior of such an organism as a rat may be divided into two phases—an active and a quiescent. If any extensive activity is prohibited during part of a day, the remaining part shows a greater "density" of activity per unit time. If the period of confinement is

made sufficiently long, the resulting active period may assume certain continuous properties.

Sample records are given showing (1) the normal curve for a "compressed" period, (2) the effect of the presence of a conditioned hunger cycle, and (3) the effect of friction upon the rate of running.

The use of the term "spontaneous" implies the existence of an internal stimulus, which can act, however, not to elicit running but only to facilitate it. The inference of such an internal factor is based upon certain characteristics of the observed behavior. Its nature as a stimulus is here questioned.

The present result is compared with earlier data on rates of eating.

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LA MESURE DE "L'ACTIVITÉ SPONTANÉE"

(Résumé)

La cage "en forme de roue" mesure une forme choisie de l'activité spontanée. Le comportement dépend en partie des propriétés de la roue. On discute la mécanique de courir dans une roue. En déterminant les spécifications d'une roue adéquate, on fait l'effort de réproduire le plus fidèlement que possible les conditions de courir sur une surface droite unie. On décrit une roue adéquate. La méthode d'enregistrer donne un graphique continu de distance et de temp du comportement.

On peut diviser le comport ent d'un organisme tel qu'un rat en deux phases—une active et une tr uille. Si l'on défend une activité étendue pendant une partie d'un jour a partie qui reste montre une plus grande

"densité" de l'activité pour le temps unitaire. Si la période d'emprisonnemént est assez longue, la période active qui résulte peut montrer de certaines propriétés continues.

Comme exemples, ont donne des résultats qui montrent (1) la courbe normale d'une période "comprimée", (2) l'effet de la présence d'un cycle de faim conditionnel, et (3) l'effet de la friction sur la vitesse de courir.

L'emploi du terme "spontanée" implique l'existence d'un stimulus intérieur, qui ne peut agir, d'ailleurs, pour causer l'action de courir mais seulement pour la faciliter. La supposition d'un tel facteur intérieur est basée sur certains traits du comportement observé. Ici on met en doute sa nature comme stimulus.

Ce résultat est comparé aux données antérieures sur les vitesses de manger.

SKINNER

DIE MESSUNG DER "SPONTANEN TÄTIGKEIT"

(Referat)

Durch das "Laufrad" ["running-wheel"] wird eine besondere Form der spontanen Tätigkeit gemessen. Die Tätigkeit ist teilweise von den Eigenschaften des Rades abhängig. Es werden die Mekanismen des Radlaufens besprochen. Bei der Notierung der Anforderungen [specifications] eines Normalrads [standard wheel] hat man versucht, so genau wie möglich die Umstände des Laufens auf einem flachen Pfad nachzubilden. Es wird ein geeignetes Rad beschrieben. Die Methode der Notierung liefert eine fortlaufende graphische Registrierung des Zeit-Distanzverlaufs [time-distance graph] der Tätigkeit.

Die Tätigkeit eines Organismus wie die Ratte lässt sich in zwei Phasen teilen,—eine aktive und eine ruhende. Wenn, während eines Teiles des Tages, ausgedehnte Tätigkeit verhindert wird, weist der übrige Teil des Tages eine dichtere Konzentrierung [greater "density"] der Tätigkeit per Zeiteinheit auf. Wird die Zeit der Einschränkung genügend lang ausgedehnt, so känn die folgende aktive Periode gewisse Eigenschaften der Beständigkeit annehmen [assume certain continuous properties].

Als Beispiele werden einige Registrierungen vorgelegt, durch welche (1) die Normalkurve einer Periode der "Einschränkung" ["compressed" period], (2) die Einwirkung eines bestehenden bedingten Hungerkreises [conditioned hunger cycle], und (3) die Einwirkung der Reibung auf die Schnelligkeit des Laufens aufgewiesen, werden.

Der Gebrauch des Ausdrucks "spontan" besagt das Bestehen eines inneren Reizes, der aber nicht das Laufen bewirken, sondern es nur erleichtern, kann. Die Folgerung einer solchen inneren Einwirkung stützt sich auf gewisse Eigenschaften der beobachteten Tätigkeit. Ihre Beschaffenheit als Reiz wird hier in Frage gestellt.

Der gegenwärtige Befund wird mit früheren Befunden in Bezug auf die Schnelligkeit des Essens verglichen.

SKINNER

STUDIES IN RETROACTIVE INHIBITION: I. THE TEMPORAL COURSE OF THE INHIBITORY EFFECTS OF INTERPOLATED LEARNING*[1]

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John A. McGeoch

PROBLEM

The inhibitory effects of interpolated learning may, *a priori,* take any one of three different courses through subsequent time. (1) They may disappear, either gradually or suddenly, so that the retention curves for the rest and for the interpolated learning, or work, conditions finally coincide. (2) Their amount may remain a constant or (3) it may increase.[2] Which one of these alternatives retroactive inhibition[3] follows is important both for an interpretation of the character of the phenomenon itself and for an understanding of the part which it plays in mental organization.

Experiments upon the problems of retroactive inhibition have commonly examined the phenomenon over intervals of time as short as 10 or 20 min., and where longer intervals have been employed no comparative records at shorter ones have been taken, so that no conclusions can be drawn regarding the temporal course of the decrement. The problem of the present experiments was the determination of the course of retroactive inhibition after intervals of 20 min., 1 hr., 24, 48, and 144 hrs. It resolves itself into comparative measurements of the retention curves obtained with and without immediately interpolated learning.

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[1] This experiment has been assisted by a grant from the National Research Council.

[2] It is, also, possible to have any number of combinations of these three, but a statement of the three major possibilities is all that is necessary at present.

[3] It should scarcely be necessary to state that the words "retroactive inhibition" are used as equivalent to the expression "inhibitory effects of interpolated activity."

METHOD

The materials to be learned were lists of 10[4] adjectives, which were exposed at a 1.8-sec. rate on an electrically driven drum and learned by the anticipation method. The subjects, who were college students and chiefly sophomores, had no prior experience with the anticipation method and were naïve with respect to any experimental work upon memorizing. In the first experiment 100 subjects were used, distributed in a manner to be described presently. On a day prior to the learning of his first experimental list, each subject was given practice at 2 lists of adjectives, each of which was learned to a criterion of 3 perfect trials with an interval of 3 min. between lists. At the beginning of practice detailed instructions[5] were given and the subject was informed, in so far as it is possible by verbal description, of the use of the anticipation method. Any departure from strict adherence to instructions was corrected during the learning of the practice lists and, by the time the subject had mastered these lists, the method had become virtually automatic. Occasional lapses during the experiment were called to his attention by the experimenter, usually in the 5-sec. interval between repetitions.

The retroactive inhibition procedure requires a rest and a work condition at each interval used and these two conditions may now be characterized. Under the *rest condition* the subject learned a list of adjectives to a criterion of 3 successive perfect trials and at once turned to reading jokes, with the instruction to mark the three best on each page of the volume read. The reading of jokes is not, of course, complete "rest," but with subjects untrained in rigorous experimental precaution some method, other than instructional, must be employed to prevent rehearsal and to give standard conditions. The subjects were made to believe that the marking of the jokes was an integral part of the experiment and their behavior uniformly indicated reaction to the jokes rather than rehearsal. During the 20-

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[4] An eleventh word was added to the list as a cue, but was not itself learned as a response.

[5] In addition to description of the method, the subject was cautioned to observe the following: (1) to react to each item as it came and to make as nearly as possible equal effort to learn each succeeding item: (2) to avoid harking back to preceding items, i.e., rehearsing, either during or between trials; (3) to make each anticipation during the exposure of the instigating member; (4) to check the correctness of each anticipation by the item when it appeared in the exposure window; and (5) to attempt actively to anticipate each item regardless of past successes and failures.

min. interval the subjects read throughout. At the longer intervals, 1 hr., 24, 48, and 144 hrs., they read for 10 min. after learning and for 10 min. before the beginning of relearning. This maintained constant conditions for the periods immediately following learning and prior to relearning at all intervals. The subjects left the laboratory and went about their usual activities during all save the 20-min. interval, returning 10 min. before the end of the interval to read jokes and then to relearn to 3 perfect repetitions in succession.

Each time, after 40 of the 60 subjects working under the rest condition had relearned, they were given 10 learning repetitions and, after 5 min., 2 relearning repetitions, of another list so that their opportunities for practice would be exactly equal to those of the work group who had, in addition to practice upon the original lists, 10 repetitions of an interpolated list. With one rest group of 20 subjects this balance list was not included. The results have been completely worked through both including and excluding this group and, since its inclusion introduces no alteration of tendency, it has been added in to increase the stability of the values under rest.

Under the *work condition* each subject learned a list to a criterion of 3 perfect repetitions and, after a 30-sec. interval, in which the experimenter prepared the drum for the exposure of another list, was given 10 presentations of a second, interpolated list. Immediately thereafter he read jokes as did the subjects under the rest condition, reading throughout the remainder of the 20-min. period. At the longer intervals the subjects read jokes for 10 min. following the interpolated lists and for 10 min. just before the relearning of the original list. All relearning was to a criterion of 3 perfect trials in succession, and following the attainment of this criterion two relearning repetitions of the interpolated list were given in order to prevent a suspicion that it was not necessary to learn it in the same way as the original list was learned, i.e., actively and with intent to retain.

There is always, in experiments such as this one, the potential variable of intervenient rehearsal. It can be guarded against only by detailed and urgent instructions. Since the subjects would be able, after the first relearning, to infer that each list would be relearned, it was necessary to tell all subjects at the beginning that each list was to be relearned and, after the learning, to caution them against rehearsal. Instructions for this purpose, prepared on the basis of the effectiveness of past attempts to control rehearsal, were memorized by each experimenter and given to each subject after each learning. After the subject had relearned, the experimenter asked as casually as possible, in order not to put the subject on his guard against saying so if he had rehearsed, whether he had thought, told anyone, or been told by anyone about the items learned or about the experiment. Cases about whom there appeared to be doubt were discarded, but care was taken that these subjects should not know that they had been discarded, lest they suspect blame and warn others against admitting rehearsal when it occurred.

Records were kept of each anticipation or failure to anticipate, so that there is available, in addition to the number of trials required for learning, the total number of anticipations made and the derivative mean number per trial. Retention has been scored in 4 different ways: (1) number anticipated on the first relearning trial, or recall-score 1; (2) number anticipated on the second relearning trial, or recall-score 2; (3) saving to a criterion of 1 perfect relearning trial, or saving-score 1; and (4) saving to a criterion of 3 perfect trials, or saving-score 2. In finding saving scores, whether from means or from individual scores, the 3 criterial learning trials have been included in the denominator, but the criterial relearning trials have not been included in the numerator. In the tables mean trials to learn do not, however, include the criterial trials. Saving scores may be computed either from the mean learning and relearning scores of the total group or separately for each individual. In the latter case the mean of the individual scores is found. Both methods have been used in this experiment. Computation from individual scores has the disadvantage that it permits extreme cases to have disproportionate weight. Suppose that a subject reaches the criterion of learning in 5 trials but requires 10 trials to relearn. His saving score of —100 will depress the mean of the individual saving scores much more than the trial scores will affect their means. Such extreme cases are few but they occasionally occur and others, less extreme, frequently appear. The advantage of this method, that it permits direct computation of sigmas from the saving scores in question, is somewhat attenuated by the extreme scores just discussed. Since, however, the two methods yield concordant results in this experiment, further comparisons are not pertinent. Percentage of retroactive inhibition has been found by the usual formula: rest score minus work score/rest score. The more important measures, from the standpoint of degree of retroactive inhibition, are recall 1 and saving 2, and discussion will confine itself primarily to them, using the two others as supplementary measures.

Sixty subjects, in 3 groups of 20 each, went through the rest condition at the 5 intervals, and 40 subjects in 2 groups of 20 each went through the work condition. The intervals were taken in an order which counterbalanced practice within each group and which also counterbalanced frequency of list per interval. The same lists were used by every group. Two experimenters were in charge of one rest and one work group each and a third experimenter was in charge of the remaining rest group. These experimenters had varying amounts of prior training in experimental research, but all had some and all were majors in psychology. They were given, previous to the beginning of the practice lists with the first subject, careful training by the writer in every detail of the present experimental conditions; they were given practice upon subjects not to be used in the experiment; and before they were permitted to begin the experiment each was required accurately to take the writer and one other person familiar with the method through every detail of a sample condition. Throughout the experiment their work was watched and was discussed with them.[6]

In the experiment described, which is the major one and which will be called Experiment I, different subjects were used under the rest than under the work conditions. As a check upon the results thus obtained, Experiment II was performed by a single experimenter with intervals of 20 min., 24 and 144 hrs., in which 12 subjects were put through both rest and work conditions at each interval in a counterbalanced order. The group is, thus, its own control. The conditions, otherwise, were identical with those of Experiment I. In both experiments, in order to decrease unmeasured interference effects, only a single learning or relearning occurred at a single sitting or during the same day, except when the conditions intrinsically required it. The 20-min. and 1-hr. intervals necessitate both learning and relearning on the same day. For the longer ones, the subject learned on one day, relearned on another, learned the next list on a subsequent day, and so on. Between the relearning of one list and the learning of another no fixed time was set, but the interval was usually one or two days. At the longer intervals a variation of plus-or-minus one hour from the exact interval was allowed; if a subject varied somewhat more than this, he was balanced by one who was asked to return at a time which varied by an equal amount in the other direction. The experimental periods under the two conditions and at the various intervals were distributed over the day from 8:00 A.M. to 6:00 P.M. in a fashion designed to avoid differential effects from diurnal variations.

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[6] The uniformity of the results obtained by these different experimenters, each of whom worked in complete ignorance of his own and of the others' results until the end of the experiment, is shown in another paper. Cf. McGeoch (6).

RESULTS

Experiment I. It is necessary, before proceeding to compare measures of retention, to ascertain whether the learning records of the two major rest and work groups are equivalent and whether within each group there is a corresponding equivalence at the different intervals. Differences in retention are confidently interpretable only if differences in learning have been negligible. Since all lists are equal in length, since all have been learned to the same criterion, and since all trials are equal in temporal length, trials are legitimate units. It may be seen from Table 1 that, under the rest condition, the means vary relatively little. The variation from interval to interval under the work condition is somewhat greater, but there is no evidence of any systematic variation and the largest difference, that of 3.10 trials between the 20-min. and 48-hr. conditions, has a sigma of 2.03. There are no marked differences between the two conditions when they are compared interval by interval, although at each one except 20 min. the work condition has slightly the smaller mean. While differences exist both within and between conditions, there is no evidence of any systematic tendency which

TABLE 1

TRIALS REQUIRED TO LEARN: MEANS AND SIGMAS

			Interval before relearning				
Condition	N		20 min.	1 hr.	24 hrs.	48 hrs.	144 hrs.
Rest	60	M	17.40	17.23	17.51	16.45	17.55
		σ_M	1.27	1.37	1.27	1.28	1.43
Work	40	M	18.20	16.30	16.60	15.10	16.67
		σ_M	1.58	1.35	1.33	1.28	1.33

TABLE 2

RECALL-SCORES 1

	Interval before relearning				
	20 min.	1 hr.	24 hrs.	48 hrs.	144 hrs.
Rest	6.26±.38	4.70±.38	2.11±.34	1.05±.21	.28±.07
Work	3.30±.39	2.70±.34	.67±.17	.47±.18	.12±.07
Difference	2.96±.54	2.00±.51	1.44±.38	.58±.27	.16±.10
Rel. R. I.	47.4	42.5	68.2	55.2	57.1

could affect the results. The mean anticipations per trial are, for the 5 intervals under rest, 6.24, 6.23, 6.0, 5.98, and 5.95; they are, correspondingly, under work, 5.95, 5.71, 6.08, 6.06, and 5.88. Again there are variations but without uniform tendency, and it is permissible to assume that the retention records have not been importantly warped by differences in learning.

In order that the results may be interpretable, the degrees of learning of the interpolated lists at the 5 intervals should be a constant. Since every interpolated list was given 10 repetitions, the test of this equation in terms of trials must be a comparison of mean numbers of anticipations per trial. These are, for the 5 intervals, in ascending order of length, 5.8, 5.6, 5.6, 5.7, and 5.5.[7] They differ but little and, if differences of this magnitude affect degree of retroactive inhibition, no method of measurement at present known is likely to be able to detect their influence. The degrees of interpolated learning are, thus, equated in terms both of trials and of mean anticipations per trial.

Given degrees of original learning which, at the different intervals under both rest and work, are free from systematic or important variation, and given degrees of interpolated learning of which the same may be said, the significant remaining variable is the interval between learning and relearning. We wish to know the comparative histories of lists learned under rest and under work over the intervals of 20 min., 1 hr., 24, 48, and 144 hrs. and shall begin with an examination of the number of items recalled upon the first relearning trial, recall-score 1 (Table 2).

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[7] Similar data have been obtained for the balance lists learned to 10 repetitions after each relearning by 40 subjects under the rest condition. The mean anticipations per trial are 5.9, 5.8, 5.2, 5.8, and 5.6.

The sigmas of the scores for rest and work are sigmas of the means.[8]

The recall scores, or number of anticipations, which stood, of course, at 10 immediately upon the attainment of the criterion, fall under both conditions with negative acceleration and without inversion, until after 6 days negligible amounts are recalled. Under the rest condition only 10 subjects out of 60 recalled anything at all after this interval, while under work the number was 3 out of 40. The decrements in saving, computed from mean scores, are much less in absolute amount (Table 4), but they occur with negative acceleration and with only one slight inversion under the work condition. Since these facts are what one would expect from the experimental data upon the curve of retention, we may proceed to examine the differences between the two conditions.

The problem makes it legitimate to look at these differences in two ways: (1) as absolute differences between scores under rest and under work, which will reveal whether the retention curves converge, diverge, or remain parallel, and (2) as relative differences. The latter is the usual way of treating data when retroactive inhibition is sought for, since in most instances absolute differences are not directly comparable, and is here the more important.

Under both conditions the curves start from complete mastery, but without exception the curve for recall under rest runs at a higher level than does the one for work, i.e., retroactive inhibition consistently appears at all intervals.[9] The absolute differences in recall score grow less with time; the curves for rest and work, thus, converge. Relative differences show no consistent tendency, although those at the longer intervals are larger than those at the shorter. If the larger percentages of inhibition at the longer intervals could be taken at their face value, it could be concluded at once that the amounts of inhibition are approximately the same after 20 min. and 1 hr., and larger by variable amounts after 24, 48, and 144 hrs. Three facts prevent such a conclusion. (1) In the first place, the differences between rest and work after 48 and 144 hrs. are very

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[8] The medians of these and of the other arrays which form the data of this paper are, for the most part, lower than are the means, but the general character of the relationships involved is the same whichever measure of central tendency is employed.

[9] It is unnecessary to present the curves themselves, since everything which they show is apparent from the tabular data.

TABLE 3

PERCENTAGES OF SUBJECTS HAVING ANY RECALL SCORE

	Interval before relearning				
	20 min.	1 hr.	24 hrs.	48 hrs.	144 hrs.
Rest	88.3	86.7	61.7	48.3	16.7
Work	80.0	82.5	42.5	27.5	07.5
Difference	8.3	4.2	19.2	20.8	09.2
Rel. R. I.	09.4	04.8	31.1	43.0	55.1

unreliable and the difference after 24 hrs. is questionable. (2) The second consideration is contained in Table 3, which gives the percentages of the cases showing any recall score. The number recalling anything at all decreases as interval increases, becoming relatively small after the three longer intervals and extremely small after 144 hrs. Unreliable differences obtained from small percentages of the population subjected to the experimental conditions permit no conclusions which rest upon such differences. (3) If recall-score 2 were to give results in clear agreement with those from recall-score 1, this uniformity would strengthen the implications of the latter, especially in view of the fact that almost all subjects recalled at least one item on the second relearning trial, but the results of the two recall scores are not concordant. The percentages of retroaction in terms of recall on the second trial are, for the 5 intervals, 22.2, 09.2, 23.9, 11.6, and 29.8. The 20-min., 24-hr., and 144-hr. intervals are relatively close together in amount of inhibition, while the 1-hr. and 48-hr. intervals have much smaller decrements. The only conclusion warranted from the examination of the data at the first and second relearning trials is that, when all cases are considered in the averages, the percentage of retroactive inhibition varies somewhat from interval to interval, but shows no tendency to vary uniformly in any single direction.[10]

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[10] If, on the other hand, one computes inhibition in terms of the percentage of cases showing any recall, the absolute differences fluctuate and the relative differences increase from 1 to 144 hrs. The relationships become still more intricate if we divide total recall scores, not by the total number of cases as has been done in Table 2, but by the the number actually recalling at least one item, i.e., by the numbers in Table 3. When this is done, the differences rapidly grow less with increasing interval (4.25, 2.12, 1.85, .45, and .04) and the relative inhibition also becomes less, although irregularly. The percentages of inhibition are 59.9, 39.1, 53.9, 20.7, and 02.4. These data mean that, while there is an increasing relative disparity in the num-

Differences between saving scores for relearning to a criterion of 3 perfect trials (Table 4), whether these scores are computed from the means of the trials or from the individual records, show no uniform increase or decrease with interval. This is true of both absolute and relative differences. In the light of their sigmas, the differences become inconsequential. The higher percentage of inhibition after 144 hrs. is produced by a few subjects who required very large numbers of trials to get from a criterion of 1 perfect trial to a criterion of 3 perfect and, in view of the large σ_D, is not important. The percentages of retroaction in terms of saving to the lower criterion are 06.8, 06.2, 05.4, 02.5, and 03.5. They reinforce the conclusion that saving scores yield amounts of retroaction which do not vary with interval.[11]

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ber of subjects able to recall anything as interval increases, in the scores of those subjects who do recall anything at all relative inhibition decreases. The means of Table 2, which are obtained by the usual method of averaging in terms of all cases, represent a composite of the two tendencies just mentioned. As a result they show no clear tendency and can probably best be interpreted as not varying demonstrably with time interval under the conditions of this experiment. The decrease in the retroaction shown by the subjects who do recall anything is not, however, to be taken too seriously. At the longer intervals the absolute amounts recalled under either condition are small and when means are found in terms only of the subjects who recall something, since the minimum recall measurable is one item and since at the three longer intervals much smaller percentages show a recall under work than under rest, the data are loaded for just this result. The mean must always be at least 1 and under work it remains between 1.0 and 1.72 at these three intervals, not rising far above the minimum amount possible, while recall under rest decreases steadily. If, however, these differences are treated as differences between percentages failing to show any recall instead of between those showing it and are divided by the percentages thus failing under rest, no regular tendency appears, although the three middle intervals have lower relative inhibition than does the 20-min. interval while the 144-hr. interval has only 11%. Since we are interested, when studying the problem, primarily in amounts actually retained and in subjects showing retention, it might be legitimate, if the matter were important, to give more consideration to the differences between the percentages of subjects actually showing a recall. By this measurement relative inhibition increases with time interval. We need not, for the reasons mentioned in the last paragraph, give these computations any weight.

[11] The relatively small percentages of inhibition in terms of saving score are a result of the fact that original learning was to complete mastery, while interpolated learning was to 10 repetitions. When nonsense syllables are learned to 16, 21, and 26 repetitions, 11 repetitions of an interpolated list produce small amounts of inhibition (Cf. McGeoch, 4), and the same may be expected for word lists. Learning to complete mastery was required in the present experiment in order to make sure of at least some recall after the longer intervals.

TABLE 4

Relearning-Scores 2 and Saving Scores

	Interval before relearning				
	20 min.	1 hr.	24 hrs.	48 hrs.	144 hrs.
			Trials to relearn		
Rest	3.06±.38	5.13±.38	6.81± .52	7.11±.49	8.68± .55
Work	4.52±.40	6.07±.62	7.40± .86	6.52±.54	10.17±1.28
Difference	1.46±.55	.94±.73	.59±1.00	+0.59±.73	1.49±1.39
			Saving scores (from means)		
Rest	85.0	74.6	66.7	63.4	57.7
Work	78.6	68.4	62.2	63.9	48.2
Difference	6.4	6.2	4.5	+ .5	9.5
Rel. R. I.	07.5	08.3	06.7	+ 0.7	16.4
			Saving scores (from individual records)		
Rest	82.3±2.36	70.8±2.49	59.6±3.54	60.7±2.88	47.4±3.77
Work	74.4±2.84	65.4±3.45	57.1±5.16	57.7±4.05	42.2±7.02
Difference	7.9±3.69	5.4±4.25	2.5±6.25	3.0±4.96	5.2±7.97
Rel. R. I.	09.6	07.6	04.2	04.9	10.9

Still another mode of analysis is to find the mean numbers of anticipations per trial at relearning after each interval and to divide each mean by the corresponding mean at learning, thus obtaining two sets of ratios, one for rest and the other for work. Relative retroaction may then be obtained by the usual method of subtracting work from rest and dividing the difference by the ratio for the rest condition. This has been done with a result confirmatory of those for saving scores and, since the method is of secondary importance, the data will not be presented. Under rest the ratios steadily decrease with increasing interval; under work the decrease is smaller and more irregular. The relative inhibition shows no tendency to vary with interval and may be considered as unaffected by interval.[12]

Experiment II. The data of this experiment, in which 12 subjects went through both rest and work conditions at the 20-min., 24- and 144-hr. intervals as a check upon the preceding one which employed

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[12] The relative variabilities of the retention records, although irrelevant to the major problem of this paper, are of interest. They can be computed from the data of Tables 2 and 4 and only a brief mention need be given them. The relative variability of recall scores is of little significance on account of the decreasing numbers of subjects represented as interval increases. That of trials to relearn to 3 perfect varies irregularly, but the V of saving scores computed from individual records increases with interval and is higher at every interval for work than for rest.

TABLE 5

RECALL-SCORES 1 AND 2: MEANS AND SIGMAS

	Recall-Score 1			Recall-Score 2		
	Interval before relearning					
	20 min.	24 hrs.	144 hrs.	20 min.	24 hrs.	144 hrs.
Rest	7.0±0.56	2.0±0.47	0	9.0±0.43	7.0±0.59	3.8±0.52
Work	2.7±0.80	1.2±0.34	0	6.8±0.61	3.8±0.89	3.9±0.71
Difference	4.3±0.98	.8± .58	0	2.2±0.75	3.2±1.07	+0.1± .88
Rel. R. I.	61.4	40.0	0	24.4	45.7	+02.6

different groups under rest and work, will be presented before any attempt is made to interpret the results of Experiment I. The trials required to learn at the 3 intervals under the rest condition were 14.5, 16.3, and 13.9, and under the work condition were 15.2, 16.0, and 11.0. The mean anticipations per trial varied in the same relative manner. The differences are larger than it is desirable to have, but since both trials and anticipations for rest and work at each interval are rather close together in magnitude, although they vary from interval to interval, the two conditions may fairly be compared. The numbers of correct anticipations during the interpolated learning were 5.69, 6.20, and 5.61.

Recall-scores 1 and 2 are given in Table 5. In the rest condition the percentages of subjects giving recall scores of one or more items were, for the 3 intervals, 100, 75, and 0; in the work condition, the percentages were 75, 41.6, and 0. Table 6 reports the data for relearning to 3 perfect trials and for saving when relearning is to a criterion of 3 perfect repetitions. The saving scores for relearning to a criterion of one perfect trial yield percentages of retroaction, at the three intervals, of 08.0, 08.8, and 06.2.

The unreliability of the difference between recall-scores 1 under rest and work after 24 hrs. supports no conclusion except that there is no reliable difference in retroaction after 20 min. and 24 hrs. This is confirmed by the reversal of the obtained relation when recall-scores 2 are considered. The absence of inhibition in recall-score 2 after 144 hrs. is consistent neither with the results from saving nor with those of Experiment I and may safely be disregarded. At the two intervals for which comparisons are possible, percentages of inhibition in terms of number of subjects showing a recall agree with those of Experiment I, as do similar percentages for the means computed for these subjects only. Saving scores agree with those of

TABLE 6

RELEARNING-SCORES 2 AND SAVING SCORES

	Trials			Saving		
	Interval before relearning					
	20 min.	24 hrs.	144 hrs.	20 min.	24 hrs.	144 hrs.
Rest	2.0±0.40	4.5±0.54	7.4±0.98	88.5	76.6	56.1
Work	4.1±0.69	7.5±1.54	6.6±0.98	77.4	60.5	52.8
Difference	2.1±0.80	3.0±1.63	+0.8±1.39	11.1	16.1	3.3
Rel. R. I.				12.5	21.0	05.8

Experiment I in showing no tendency to vary with interval. This is true, also, when saving scores are found for relearning to one perfect trial, and when individual scores for relearning to both criteria are computed.

Summary and Discussion

It is axiomatic that, in a problem of this kind, a conclusion that retroaction varies with interval should rest upon data of uniform tendency and statistical reliability. Differences must be expected, but lacking consistency and reliability these differences can be ascribed only to the chance variations to which all experimental work upon learning is subject, plus the added variable of intercurrent experience over different intervals of time. The saving scores of both experiments have yielded amounts of inhibition which vary irregularly over a small range and which consistently fail to show any regular change with increasing interval. The conclusion is well supported that percentage of inhibition is approximately a constant over these intervals. The inhibition to which recall scores are subject is less uniform, but when all subjects are included in the computations and when both experiments are considered, no consistent tendency to vary with time is discernible. If, however, inhibition is viewed from the standpoint of complete failure to recall and is found in terms of the percentages of cases showing any recall, it increases with interval. Although relative differences are the more important, completeness of analysis requires an examination of the course of the absolute values. In terms of recall-scores 1, absolute differences steadily grow less to give converging curves. In terms neither of percentages of subjects having any recall score nor of any method of measuring saving do the curves show any uniform tendency to vary together.

The fact of the relative constancy of degree of inhibition over intervals varying from 20 min. to a week is of importance to an interpretation of forgetting. The writer has elsewhere discussed the evidence that retroactive inhibition is one of the major conditions of forgetting (5). It is necessary, however, for any exact formulation of the effect of interpolated events, to know much more about the phenomenon than we know at present. These experiments were undertaken in order to determine (1) the temporal course of forgetting for some time after the interpolated learning has ceased and (2) thus to find whether or not the facts agree with the usual phenomena of curves of forgetting. The usual curves for lists of serial items fall rapidly at first and then more slowly, finally approaching the abscissa very gradually. It would be possible, theoretically, for the combined effects of interpolated learning and everyday experience to be cumulative and, thus, to yield a curve which would fall very much faster than the typical Ebbinghaus curve obtained without special measured interpolation. If this had turned out to be the case, retroactive inhibition as a major condition of forgetting would have been in danger of explaining too much. Had the inhibitory effects of the interpolated learning tended to disappear with time, a sharp limitation would have been placed upon inhibition from interpolated learning and it would have had a greatly lessened value as a condition of forgetting. The results obtained, that the curve under work has a fall proportional to that under rest, are consistent with what is known about curves of forgetting and, while they could not have been predicted with any assurance from either fact or theory, are perhaps the most favorable among the possible results to the theory that retroactive inhibition is a major condition of forgetting.

While the first concern of these experiments has been with the facts, interpretation in the light of current theories of retroactive inhibition will cast critical light upon the theories themselves. The perseveration theory asserts that interpolated learning interferes with the continuation, neural or otherwise, of the activity involved in the original learning, activity which would normally go on after that learning has formally ceased, and that, as a result of this diminution or prevention of perseveration, retention suffers a decrement relative to that which follows a condition of rest. This is tantamount to a statement that material less well learned will be less well retained, since the sole assumed effect of the perseveration is a better fixation of the material. If perseveration is blocked by interpolated learning, the antecedent activity which is, thus, denied the fixating effect must be assumed to start its course through time, under most conditions of original learning, with a less well-fixed organization than does the material which was followed by rest. If no other factors than those implied by the perseveration theory are present, either the curves of retention might be parallel while percentages of difference increase or the percentage differences might not vary. From the theory alone it is impossible to say which alternative to expect. There is, however, a considerable body of experimental fact to indicate that, whether perseveration is operative or not, there are present other factors of the first importance, factors which are subsumed under the transfer theory.[13] Since the perseveration theory is indeterminate with respect to the point at issue, we may turn to the transfer theory, by which the facts of these experiments are better interpreted.

The usual formulations of the transfer theory of retroactive inhibition are silent upon the point which is of first concern in determining its relation to the present results, viz., upon the temporal locus of the confusion which is supposed to follow from the transfer between original and interpolated lists, or upon the temporal locus of the inhibition. We shall state the possibilities in this regard and examine each in the light of the data reported.

1. The transfer and resulting inhibition may occur during the learning of the interpolated list. The inhibition in this case may operate in either one of at least two different ways. (*a*) It is possible that the integration of the original list is weakened immediately and has from that point onward a poorer organization than has the list learned under the rest condition. It may, therefore, be expected to be more susceptible to the inhibitory effects of everyday experience than is the list the learning of which was followed by "rest." These effects may be expected to be cumulative, and relative degrees of inhibition should, then, increase with time. The rate of this increase cannot be predicted from this version of the theory, but even if the amount of the immediate disintegration remained unaltered, the relative inhibition should increase. Perseveration theories plus the inhibition from intercurrent experience would yield the same expectation. The absolute amounts of inhibition vary. In so far as they show any tendency it is to decrease with time, but relative inhibition shows a general tendency to remain constant. This form of the transfer theory, together with perseveration plus transfer, are, therefore, not supported.

(*b*) As the view has been stated above, the organization of the original activity has been weakened by the interpolated activity and it is assumed that the weakening is permanent. It might be argued, however, that there is no real weakening of organization, but that there is, instead, a confusion between items which becomes less with time as the interpolated or confusion items themselves become forgotten. They may be expected to be forgotten more rapidly than the original items because they were less well learned. In this experiment, it might be urged, they disappear fast enough to keep the percentage of inhibition roughly a constant. Intercurrent everyday experience would operate equally upon the materials learned under rest and under work, while the inhibition from the interpolated learning drops out as the confusion items (the members of the interpolated list) are forgotten. This form of the view that the temporal locus of transfer is during the interpolated learning is a conceivable interpretation of the present results. There are, however, certain facts which render its accuracy doubtful. In the first place, it implies that the absolute differences between rest and work conditions should decrease with increasing interval, i.e., as interfering associations disappear. While recall scores fulfill this expectation, saving scores do not. In the second place, interpolation immediately prior to relearning, instead of just subsequent to original learning as in these experiments, should yield absolute differences which increase with interval, because the integration of the original list is less at the time of interpolated learning as the interval grows longer. During the interpolated learning, then, the original list is more susceptible to interference after the longer intervals, while the degree of learning of the interpolated lists is a constant. As a part of this investigation, to be reported separately, the influence of interpolation just prior to relearning after each of the five intervals has been studied. The data show no such increase in either absolute or relative differences, nor do they show the very much greater inhibition at this position of interpolation which the view under discussion would lead one to anticipate. The position just prior to relearning is usually the more productive of inhibition, but not usually by very large amounts. The possibility that the locus of transfer is during the interpolated learning does not appear a likely one from the data of these experiments.

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[13] See, e.g., Robinson (8), Skaggs (10), Robinson (9), Whitely (12), Harden (2), Cheng (1), and McGeoch and McDonald (7). The fact that the curves of retention reported by Jenkins and Dallenbach (3) and by Van Ormer (11) are flat after two hours spent in sleep, while they continue to fall from that point to eight hours during waking, strongly supports the same general conclusion.

2. It is possible, in the second place, that the action of the interpolated activity is distributed in some manner over a part or all of the interval. If this were the case, inhibition should increase with

interval and against this inference the considerations advanced against the first possibility apply. It seems at present to be unlikely.

3. According to the third possibility, the temporal locus of negative transfer is during the measurement of retention. If it be assumed that original and interpolated lists are forgotten during the interval at proportional rates, it is reasonable on this view of the locus of transfer to expect that inhibition should remain a constant. The reasonableness of this view depends upon the assumption that proportional rates will yield constant amounts of inhibition. The necessary facts for an evaluation of the three possibilities are not available, but at present the third possibility is the most acceptable.

The facts of these experiments, as must be evident from the discussion, raise many more problems than they solve. In particular they emphasize the necessity of an experimental analysis of the temporal locus of transfer before the transfer theory of retroactive inhibition can be stated precisely. The theory, as it stands, does no more than to state that transfer occurs between the original and the interpolated lists and that, as a result of this transfer, a decrement in retention ensues. This much is important, but it goes little beyond defining the area within which the effective conditions lie. In addition to the temporal locus of inhibition, we need to know, for example, in which direction the negative transfer occurs. Do parts of the original list transfer to the interpolated list and thus lose or weaken their places in the organization of the original? Or do the interpolated items transfer to the original list and interfere with its integration? Or do both occur? One may ask, further, whether the transfer is in terms of individual items, whether some more general influence of total integration upon total integration is present, or whether both are found. An experimental answer to these questions will assist not only in our understanding of the inhibitory effects of interpolated learning but also in an interpretation of positive transfer and of the whole organization of mental events.[14]

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[14] Nothing has been said, in this discussion of results, concerning the facts that in terms of number of subjects recalling anything absolute differences between rest and work vary irregularly, while relative differences increase. Failure to recall is greatly affected by the rough all-or-none unit of a single item which the nature of the material and of the method imposes, and data of this kind must not be taken too seriously. In so far as they are to be considered, they fit the transfer theory better than they fit perseveration and can be interpreted in the light of any of the three forms of the transfer theory, although superficially the first form fits them best.

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ÉTUDES DE L'INHIBITION RÉTROACTIVE: I. LE COURS TEMPOREL DES EFFETS INHIBITEURS DE L'APPRENTISSAGE INTERCALÉ

(Résumé)

L'expérience principal rapporté dans cet article a eu comme but la détermination des quantités de l'inhibition rétroactive après des intervalles de 20 minutes, d'une heure, de 24, de 48, et de 144 heures. Soixante sujets ont subi la condition de repos où ils n'ont rien appris dans le laboratoire dans l'intervalle entre l'apprentissage d'une liste de 10 adjectifs à un critère de trois épreuves successives par la méthode d'anticipation et le réapprentissage de la liste au même critère. Quarante sujets ont subi la condition de travail qui n'a différé de la condition de repos qu'à un seul égard: immédiatement après l'apprentissage parfait de la première liste on a fait répéter dix fois une seconde liste de 10 adjectifs. Dans une expérience de contrôle 12 sujets ont subi la condition de repos et celle de travail toutes deux aux intervalles de 20 minutes, de 24 et de 144 heures.

Les résultats de l'épargne dans les deux expériences cèdent des quantités d'inhibition lesquelles varient irrégulièrement dans une petite étendue et ne

montrent constamment nul changement régulier comme l'intervalle s'accroît. On conclut que le pourcentage de l'inhibition est relativement constant pendant ces intervalles. L'inhibition à laquelle les résultats du rappel sont sujets est moins uniforme mais nulle tendance constante à varier avec le temps ne paraît.

On interprète ces résultats en faveur de la théorie que l'inhibition rétroactive est une principale condition de l'oubli. La théorie du transfert semble en général soutenue par ces expériences, surtout cette forme de la théorie qui maintient que le locus temporel du transfert négatif est pendant la mesure de la rétention. Les expériences appuient sur l'importance d'une analyse expérimentale du locus temporel du transfert et d'autres aspects rapportés de cette théorie de l'inhibition rétroactive.

McGeoch

FORSCHUNGEN IM BEREICH DER RÜCKWIRKENDEN HEMMUNG: I. DER ZEITLICHE VERLAUF DER HEMMENDEN EINWIRKUNGEN DES INTERPOLIERTEN LERNENS

(Referat)

Es war die Aufgabe des Hauptexperimentes, worüber in dieser Arbeit Bericht erstattet wird, die Quantität der rückwirkenden Hemmung nach Zeitabständen von 20 Minuten, einer Stunde, 24 48, und 144 Stunden, zu bestimmen. Sechzig Versuchspersonen machten einen Ruhezustand durch, in dem im Laboratorium nicht gelernt wurde während des Intervalles zwischen dem Lernen einer Liste von zehn Eigenschaftswörtern mit der Voraussagungsmethode [anticipation method] (bis sie drei Mal hintereinander richtig hergesagt werden konnten) und dem Wiederlernen der selben Liste mit der selben Vollständigkeit. Vierzig Versuchspersonen machten einen Arbeitszustand durch, dessen einziger Unterschied von dem Ruhestand darin bestand, dass eine zweite Liste von 10 Eigenshaftswörtern sofort nach der Bemeisterung der ersten Liste zehn Mal hintereinander wiederholt wurde. In einem Kontrollversuch machten zwölf Versuchspersonen, nach den Zeitabständen von 20 Minuten, 24 Stunden und 144 Stunden, sowohl den Ruhezustand wie den Arbeitszustand durch.

Die in beiden Versuchen erhaltenen Ersparnisszahlen liefern [saving scores] Beträge an Hemmung die sich unregelmässig über einen kleinen Kreis erstrecken und bei denen der Beweis einer regelmässigen, mit zunehmender Länge des Zeitabstandes einhergehenden, Änderung konsequent mangelte. Es wird hieraus geschlossen, dass der Prozent der Hemmung im Bereiche dieser Zeitabstände relative konstant ist. Die Hemmung, denen die bei der Wiederholung erzielten Zahlen [recall scores] unterworfen sind, ist nicht so einförmig, aber es zeigt sich bei ihr keine Richtung danach, mit Änderungen des Zeitverlaufes zu variieren.

Diese Theorien werden als der Theorie günstig gedeutet, dass die rückwirkende Hemmung eine Hauptbedingung des Vergessens bildet. Die Übertragungstheorie scheint im Allgemeinen durch diese Experimente unterstützt zu werden,—besonders die Form dieser Theorie die behauptet, dass der zeitliche Ort [temporal locus] der negativen Übertragung während der Messung der Beibehaltung [retention] liegt. Die Experimente betonen die Wichtigkeit einer experimentellen Analyse des zeitlichen Orts der Übertragung und anderer Seiten dieser Theorie der rückwirkende Hemmung.

McGeoch

STUDIES IN RETROACTIVE INHIBITION: II. RELATIONSHIPS BETWEEN TEMPORAL POINT OF INTERPOLATION, LENGTH OF INTERVAL, AND AMOUNT OF RETROACTIVE INHIBITION*[1]

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John A. McGeoch

Problem and Method

The relationship between the temporal position of interpolated learning and the magnitude of the decrement in retention resulting from such interpolation has been, since the work of Müller and Pilzecker (5), one of the important problems in the field of retroactive inhibition. It is significant, first, as one among the conditions which may determine the degree of such inhibition and, second, as having, supposedly, almost crucial importance for the perseveration theory of the phenomenon. This paper reports an experimental comparison of the amounts of retroactive inhibition after intervals of 20 min., 1 hr., 24, 48, and 144 hrs., when interpolation occurs (1) immediately after the learning of the original list, and (2) just prior to the beginning of relearning.

In Experiment I, lists of 10 adjectives were exposed at a 1.8-sec. rate on an electrically driven drum and were learned and relearned by the anticipation method, in each case to a criterion of 3 perfect trials in succession. The rest condition, and the work condition in which the interpolated learning began just subsequent to original learning, together with the experimental controls and the methods of scoring and treating the data, have been described in a previous paper (3).[2] The same procedure has been employed in a second work condition (Experiment I of this paper) in which 20 subjects

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[1] This experiment has been assisted by a grant from the National Research Council.

[2] The reader is referred to this paper for a detailed description of the experimental conditions.

received the 10 presentations of the interpolated list just prior to relearning. The subjects returned, read jokes for 10 min., learned the interpolated list, and after 30 sec. began the relearning of the original list. In other respects the experimental conditions were identical save that this part of the experiment was conducted by a single experimenter. The data under the rest condition are for 60 subjects, those under work with immediate interpolation are for 40, and those for work with interpolation prior to relearning are for 20.

We have, then, for comparison at each of the 5 intervals, a rest condition and two work conditions, in one of which the interpolated learning began 30 sec. after the attainment of the criterion with the original list, and in the other of which the interpolated learning ended 30 sec. before the relearning of the original list began. The lengths of time free from interpolated learning are identical in the two work conditions; the conditions differ only in the temporal point at which the interpolated learning occurs. If reliable differences in amount of retroaction are found, they may validly be ascribed to whatever conditions are involved in differences in temporal point of interpolation.

Experiment II is a duplicate of the second experiment of the previous paper. Intervals of 20 min., 24 and 144 hrs. were used and 12 subjects went through both rest and work conditions at each interval. Whereas, in Experiment I, different groups are compared under rest and work, in Experiment II each subject is his own control, since each has been through both conditions in a counterbalanced practice order.

RESULTS

Experiment I. Since the data for the rest and for the first work condition have already been presented in the paper referred to, they may be assumed and the primary emphasis placed upon those for the second work condition. The mean numbers of trials required to learn at the 5 intervals of this condition, with their sigmas, are 16.85±2.00, 18.35±2.75, 14.60±1.25, 14.75±1.51, and 13.80±1.70. These means vary somewhat among themselves and these variations must be remembered when the results are interpreted. They are of the same order of magnitude as are those for the two other conditions. The differences among the means at the 5 intervals are introduced between the first perfect repetition and the attainment of 3 such repetitions in succession, since the mean trials

TABLE 1

MEAN RECALL, RELEARNING, AND SAVING SCORES
(Interpolation prior to relearning)

	Interval before relearning				
	20 min.	1 hr.	24 hrs.	48 hrs.	144 hrs.
Recall 1	1.80± .47	1.95± .56	.95± .36	.10± .09	.05± .04
Recall 2	6.40± .58	5.45± .56	5.15± .50	4.35± .57	3.10± .40
Relearning 2	6.10± 1.19	7.10± .88	6.70± .56	7.05± .62	10.50± 1.23
Saving (from means)	69.2	66.7	61.9	60.3	37.4
Saving (ind. rec.)	56.8 ±12.1	56.2 ±7.1	58.0 ±4.7	56.8 ±3.6	26.9 ±10.6

to the first one are close together, being 11.3, 10.9, 10.4, 10.8, and 10.6. The mean anticipations per trial are 6.48, 6.50, 6.46, 6.51, and 6.22. The mean anticipations per trial for the 10 repetitions of the interpolated list are not far apart in magnitude, being 5.92, 5.49, 5.47, 6.07, and 5.95.

The mean numbers of correct anticipations on the first and second relearning trials (recall-scores 1 and 2), the trials required to relearn to 3 perfect repetitions, and the saving scores computed both from these means and from the individual records, are shown in Table 1. The sigmas are of the means.

The recall-scores 1 are all small and, with one exception, decrease as interval lengthens. Recall-scores 2 are larger and decrease uniformly. The saving scores computed from mean trials decrease very slowly from 20 min. to 48 hrs. and fall sharply from that point to 144 hrs. Similar scores found for each individual and then averaged show no decrease from 20 min. to 48 hrs., but have the same abrupt drop to 144 hrs.

The important consideration in the present context is, however, the comparative percentages of retroactive inhibition yielded by this condition, interpolation prior to relearning, and by interpolation immediately following learning. The percentages from the latter position, already given in a previous paper, are reproduced for comparison with those of the present experiment (Table 2).

In terms of recall-score 1 interpolation prior to relearning is accompanied by the greater percentage of retroaction after every interval save 24 hrs. The great instability of the means and the small percentages of subjects showing any recall score, at the 24-, 48-, and 144-hr. intervals, render all of these percentages very questionable. Their tendency is, however, mirrored in that of the data from recall-score 2, except for the virtual equality of the percentages after 144 hrs. in the case of the latter. At every interval retroaction in terms of the percentage of subjects having any recall score is higher when interpolation is prior to relearning. It may be concluded that there is a tendency for this condition to be the more productive of inhibition at recall after all of the intervals studied except 24 hrs.

When saving scores are computed from the means of the trials, interpolated learning just prior to relearning is consistently accompanied by the greater inhibition. The same is true, with the exception of the 24-hr. interval, when computation is from the individual

TABLE 2

PERCENTAGES OF RETROACTIVE INHIBITION WHEN INTERPOLATION IS IMMEDIATELY AFTER LEARNING AND WHEN IT IS JUST PRIOR TO RELEARNING

Position of	Interval before relearning				
interpolation	20 min.	1 hr.	24 hrs.	48 hrs.	144 hrs.
	Recall 1				
After L	47.4	42.5	68.2	55.2	57.1
Before RL	71.2	58.5	54.9	90.4	82.1
	Recall 2				
After L	22.2	09.2	23.9	11.6	29.8
Before RL	27.5	22.6	14.8	16.8	28.7
	Percentage of subjects having any recall score				
After L	09.4	04.8	31.1	43.0	55.1
Before RL	32.0	48.1	35.1	79.3	70.0
	Saving 2 (from mean trials)				
After L	07.5	08.3	06.7	+00.7	16.4
Before RL	18.5	10.6	07.2	04.8	35.1
	Saving 2 (from individual records)				
After L	09.6	07.6	04.2	04.9	10.9
Before RL	30.9	20.6	02.6	06.4	43.2

learning and relearning records of each subject. The data for saving, when relearning is figured only to a criterion of one perfect trial, show much smaller differences between the two positions of interpolation, but the direction of the differences is, in 4 out of 5 intervals, the same as that shown by the saving scores to the more rigorous criterion. The conclusion drawn for recall scores holds more consistently still for saving scores.

Degree of inhibition may also be computed in terms of the ratios of the mean anticipations per trial at learning and relearning. The means at relearning have been divided by those at learning and these percentages have been treated in the usual way (rest minus work/rest) to determine percentage of inhibition. At every interval, save at 24 hrs. where the two are practically equal, interpolation prior to relearning is accompanied by the greater inhibition. There is no consistent tendency for the difference to vary with interval, nor is there any uniform variation in the absolute size of the ratios.

A second question is that of the relative magnitudes of the differences at the 5 intervals. It is obvious from Table 2 that at the 24-hr. interval the differences are much smaller than at the other intervals, sometimes even reversing the direction of the others. The relatively large difference at the 144-hr. interval is difficult to evaluate on account of the small number of trials required to learn at this interval, with the consequent to-be-expected tendency to a decrease in saving. On the principle, for which there seems to be considerable supporting evidence in the literature, that in such experiments retention averages an inverse relation to rapidity of learning, one would expect the relatively lower recall and saving scores which emerge both here and at the two longer intervals. Considering all of the evidence, it is impossible to say certainly that the greater retroaction following interpolation prior to relearning is more pronounced at one interval than at another, with the exception of the 24-hr. interval after which the difference is less than after any of the others and is sometimes reversed in direction.

The general trend of the percentages of retroaction supports the conclusions of the preceding paper on the temporal course of inhibitory effects. Neither recall score shows a consistent change with interval, while the percentage of subjects having any recall score increases, though not regularly. Retroaction in terms of saving scores decreases somewhat from 20 min. to 24 or 48 hrs., but, on account of the lack of uniformity in mean trials required to learn, the interpretation of the saving scores in question is difficult. The most probable conclusion is that differences in retroaction between intervals, if such differences exist, are very small.

Experiment II. The data of this experiment, in which 12 subjects went through both rest and work conditions, with interpolation prior to relearning under work, at 20 min., 24 and 144 hrs. in a counterbalanced practice order, serve as a check upon those of Experiment I. The mean trials required to learn are, for each of the three intervals under rest, 15.5, 17.2, and 17.8; under work they are 17.8, 17.0, 18.5. The corresponding mean anticipations per trial are 6.83, 5.81, 6.07, and 5.85, 5.73, 6.70. Both trials and anticipations per trial are of the same order of magnitude as are those of Experiment II of the preceding paper, but they vary over a shorter range. The mean anticipations per trial for the interpolated lists at the three intervals are 5.29, 5.46, and 5.48.

The means, with their sigmas, of recall-scores 1 and 2 appear in Table 3. Since N is 12 there is little value in reproducing the percentages of subjects showing any recall score. They decline with

TABLE 3

RECALL-SCORES 1 AND 2: MEANS AND SIGMAS

	Trials			Saving		
	Interval before relearning					
	20 min.	24 hrs.	144 hrs.	20 min.	24 hrs.	144 hrs.
Rest	7.0± .79	2.6±.46	.41±.27	8.0±.45	5.6±.64	4.3±.55
Work	2.8± .73	0.8±.07	.16±.15	6.2±.75	2.2±.45	2.5±.54
Diff.	4.2±1.05	1.8±.46	.25±.31	1.8±.87	3.4±.78	1.8±.77

TABLE 4

RELEARNING-SCORES 2 AND SAVING SCORES

	Trials			Saving		
	Interval before relearning					
	20 min.	24 hrs.	144 hrs.	20 min.	24 hrs.	144 hrs.
Rest	4.7±1.09	6.8±1.14	9.6±1.78	74.5	66.3	53.8
Work	5.3± .97	9.5±1.00	11.5±1.93	74.5	52.4	46.5
Diff.	.6±1.46	2.7±1.51	1.9±2.63	00.0	13.9	7.3

TABLE 5

PERCENTAGES OF RETROACTIVE INHIBITION WHEN INTERPOLATION IS IMMEDIATELY AFTER LEARNING AND WHEN IT IS JUST PRIOR TO RELEARNING

Position of interpolation	Recall-score 1			Recall-score 2		
	Interval before relearning					
	20 min.	24 hrs.	144 hrs.	20 min.	24 hrs.	144 hrs.
After L	61.4	40.0	00.0	24.4	45.7	+02.6
Before RL	60.0	69.2	60.9	22.5	60.7	41.8
	Saving (to 1 perfect)			Saving (to 3 perfect)		
After L	08.0	08.8	06.2	12.5	21.0	05.8
Before RL	10.5	19.3	05.2	00.0	20.9	13.5

time and give percentages of retroaction of 09.1, 90.0, and 50.0. In Table 4 are given the trials required to relearn to a criterion of 3 perfect trials and the saving scores computed from mean trials, while the relative retroactive inhibition computed from the data of Tables 3 and 4, together with the corresponding percentages from Experiment II of the preceding paper in which the interpolation followed the original learning immediately, are presented in Table 5. Data from saving scores to one perfect trial are also included.

In this experiment interpolation prior to relearning does not so consistently produce the greater relative inhibition, but there is no general reversal of the relations found in Experiment 1. Retroaction is either practically the same at the two positions or the position prior to relearning has the greater amount. The only exception to this is saving-score 2 at the 20-min. interval and in interpreting this exception one must remember the smaller mean number of trials required to learn at this interval under rest. The same is true when individual records are used in the computation of saving scores. With the conclusion stated the results from computation of inhibition in terms of the ratios of mean anticipations per trial agree. There is no consistent tendency for the differences to be larger after one interval than after another, unless it be that more inhibition of recall occurs at the two larger intervals than after 20 min.[3] On account, however, of the unreliability of recall scores after the longer intervals this cannot be granted much weight. The conclusion of the preceding paper that there is no uniform and reliable variation of retroaction with time-interval is confirmed by these results.

Discussion

The results of two experiments have shown that the positions of interpolation immediately subsequent to learning and just prior to relearning may occasionally be equal in percentages of retroaction produced, but that the position just before relearning often produces the greater inhibition. There is clearly no evidence for the superior potency of the position immediately after learning. The prediction from the perseveration theory that immediately interpolated activity should block perseveration and thus produce inhibition more strongly than should later interpolations is directly contradicted. Either the prediction does not follow from the theory or the failure of the facts to conform to the prediction must weigh against the theory. Since the prediction is justified by the usual statement of the theory, the present facts must be accepted in evidence against the theory unless the experimental procedure employed is such as to alter the interpretation.

The facts reported weigh inferentially against the perseveration theory, but they are not crucial, and it is very doubtful if crucial evidence can be obtained by a comparison of positions of interpolation. The fact that at the two positions the organization of the original activity can hardly be the same at the time of interpolation prevents comparison. When interpolation occurs immediately subsequent to original learning, the organization of the latter is at its maximum during the interpolation. When, on the other hand, interpolation is delayed until the period immediately before relearning, forgetting of the original material has had an opportunity to occur and the interpolated learning finds the original activity less well organized. The degrees of interpolated learning are equal in the two cases but those of the original material are not, and the experiment thus becomes one to measure the effect of equal interpolation with two degrees of organization of the original activity. It is reasonable to suppose that conclusions from lists of nonsense syllables learned under closely similar experimental conditions apply also to the lists of adjectives of the present experiment. In so far as this is true, inhibition should vary inversely as degree of original learning (1) and should be of some magnitude when as many as 10 repetitions are given the interpolated lists (2). We should expect, therefore, in the present instance, the position prior to relearning to yield the larger amounts of inhibition; and, by the same reasoning, the discrepancy should increase with the interval. The results fulfill the first expectation, but fail to conform to the second. In the light of the following considerations, however, this failure is not decisive. (1) While there is no uniform increase with interval in the differences between the two positions of interpolation, there is a tendency to an increase from the 20-min. interval to the two longer ones in terms of the recall and saving scores of Experiment I and in the recall scores of Experiment II, although these differences are of questionable acceptability. (2) The largest gross decrement occurs between the attainment of the learning criterion and the measurement of retention after 20 min. The decrements from interval to interval beyond this point are much smaller and it may be either that they are not sufficient to permit the degree-of-learning phenomenon in question to show itself, that it does not operate as clearly or in the same way when the interpolation is delayed and when the lessened organization occurs after the cessation of learning and not as a function of low frequency of repetitions, or that the obtained increases mentioned above are more representative than they statistically appear to be. So long, however, as the comparisons of amounts of inhibition yielded by the two positions of interpolation are potentially obscured by unequal degrees of organization of the original material, such comparisons cannot be interpreted as crucial for the perseveration theory. If blocking of perseveration were the chief or the only condition involved, however, the present results could hardly occur, so that they must be interpreted as contradicting it as a major factor, but they do not prove its entire ineffectiveness.

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[3] The fact that, in this experiment, retroaction is greater after 24 hrs., in terms of all except saving-scores 2, when interpolation is prior to relearning than when it is subsequent to learning makes questionable the interpretation of the failure of the results of Experiment I at this interval to agree with those at the four other intervals. Since this failure does not importantly affect the interpretation, no hypothesis will be offered in explanation.

Why other investigators, such as Müller and Pilzecker (5) and Skaggs (7), have found the position subsequent to learning to be the more productive of inhibition is not obvious, but differences between their experiments and the present one in degree of original learning, as well as in many other ways, may explain the differences in results. The outcome of the present experiment is more nearly akin to Robinson's (6) conclusion that there is no important difference between the two positions, while with Whitely's (8) results these are in complete agreement. This agreement may well be a function of the fact that he employed a 48-hr. interval, thus allowing time for differences in the organization of the original materials to become important. Other conditions than absolute degrees of organization of original and interpolated activities are, probably beyond doubt, operative in producing the comparative degrees of inhibition at the two positions. It is the intent of this discussion to show only that the inevitable presence of such differences in organization prevents comparisons of inhibition at different positions of interpolation from providing crucial evidence upon the perseveration theory,[4] although they do not vitiate the importance of temporal position as one of the conditions of the phenomenon itself.

The other major interpretation of the facts of retroactive inhibition, the transfer theory, is indirectly supported by this experiment, inasmuch as this theory is able to incorporate the results without difficulty. There is nothing in the usual statement of the theory which would lead one to expect more inhibition from one position of interpolation than from another. Experimental work may reveal preferential positions, but the theory *per se* assumes none. The results of the present experiment set certain problems concerning the locus and character of transfer, but these are beside the point of the present interpretation.

SUMMARY

Lists of 10 adjectives, learned to a criterion of 3 perfect successive trials, have been relearned after intervals of 20 min., 1 hr., 24, 48, and 144 hrs., each list being relearned but once and without overlapping of intervals. Sixty subjects did no other formal learning

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[4] The results of these experiments are in agreement with those from a similar experiment in which a stylus maze was learned and, after 24 hrs., relearned both with and without interpolation in both of the positions used here. Cf. J. A. McGeoch and M. E. Nolen (4).

in the laboratory during any interval; 40 other subjects learned a second list to the degree made possible by 10 repetitions immediately after the mastery of the original list; and 20 other subjects had a similar 10 presentations at the end of the interval and just prior to the beginning of relearning. In a second experiment 12 subjects went through both rest and work conditions with the 20-min., 24-hr., and 144-hr. intervals. The conclusions are summarized below.

1. By all measures of retention, retroactive inhibition tends to be greater after 20 min., 1 hr., 48 and 144 hrs. when interpolated learning occurs just prior to relearning than when it is immediately subsequent to learning. The differences at the 24-hr. interval, which are small and which sometimes reverse the above conclusion, are probably an unimportant exception. The 24-hr. interval offers no consistent exception in Experiment II, in which interpolation prior to relearning yields amounts of inhibition equal to or greater than those found at the other positions.
2. These results contradict the prediction from the perseveration theory that interpolation just subsequent to learning should be most productive of inhibition and imply that perseveration must play a minor part, if it plays any part at all, in the determination of the phenomenon of retroaction. Such experiments do not, however, prove that the blocking of perseveration is not a minor condition, because the original activity is at a lower level of organization when interpolation is prior to relearning and the conditions at the two positions are not, therefore, strictly comparable. The transfer theory is able to incorporate these results and is strengthened thereby.
3. There is no consistent increase with interval in the differences between the amounts of inhibition at the two positions, although such an increase would be expected from the interpretation given in 2. For reasons elaborated in the body of the paper, the facts upon this point are not capable of decisive interpretation.
4. The conclusion of the preceding paper that there is no uniform and consistent variation with time interval in percentage of retroactive inhibition is corroborated by the present results.

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ÉTUDES DE L'INHIBITION RÉTROACTIVE: II. LES RELATIONS ENTRE LE POINT TEMPOREL DE L'INTERCALATION, LA LONGUEUR DE L'INTERVALLE, ET LA QUANTITÉ DE L'INHIBITION RÉTROACTIVE

(Résumé)

On a fait des comparaisons entre les quantités d'inhibition rétroactive produites par l'intercalation immédiatement après le premier apprentissage et immédiatement avant le réapprentissage, avec des intervalles de 20 minutes, d'une heure, de 24, de 48, et de 144 heures. Tous les sujets ont appris une liste de 10 adjectifs par la méthode d'anticipation à un critère de trois épreuves parfaites successives et l'ont apprise encore une fois au même critère. Soixante sujets n'ont rien appris dan le laboratoire pendant les intervalles; quarante autres ont eu 10 présentations d'une seconde liste immédiatement après l'apprentissage parfait de la première; vingt autres ont eu 10 présentations de la seconde liste immédiatement avant le réapprentissage.

L'inhibition rétroactive tend à être plus grande après 20 minutes, une heure, 48 et 144 heures, quand l'apprentissage intercalé a lieu immédiatement avant le réapprentissage que quand il est immédiatement subséquent à l'apprentissage. Les différences après l'intervalle de 24 heures ne sont pas constantes, mais une expérience de contrôle indique cet intervalle n'offre aucune exception importante à la conclusion générale. Les résultats contredisent la prédiction de la théorie de la persévération et indiquent que la persévération ne joue tout au plus qu'un petit rôle dans la détermination de l'inhibition rétroactive. La conclusion d'un premier article qu'il n'existe nulle variation uniforme et constante avec l'intervalle de temps dans le pourcentage de l'inhibition est corroborée par ces expériences.

McGeoch

FORSCHUNGEN IM BEREICH DER RÜCKWIRKENDEN HEMMUNG: II. DIE BEZIEHUNGEN ZWISCHEN DEM ZEITPUNKT DER INTERPOLIERUNG, DER DAUER DER ZEITABSTÄNDE, UND DEM BETRAG AN RÜCKWIRKENDER HEMMUNG

(Referat)

Es sind Vergleichungen ausgeführt worden, zwischen den Quantitäten der gerade nach dem ursprünglichen Lernen und der gerade vor dem Wiederlernen durch die Interpolierung verursachten rückwirkenden Hemmung, bei Zeitabständen von 20 Min., einer Stunde, 24, 48, und 144 Stunden. Alle Versuchspersonen lernten eine Liste von 10 Eigenschaftswörtern mit der Voraussagungsmethode [anticipation method] bis sie drei Mal hintereinander richtig hergesagt werden konnten, und lernten wieder [relearned] bis zur Erreichung der selben Vollständigeit. 60 Vpp. führten im Laboratorium während der Zeitabstände kein Lernen aus; 40 anderen wurde sofort nach dem Lernen der ersten Liste 10 Mal eine zweite Liste gezeigt; 10 weiteren Vpp. wurde die zweite Liste 10 Mal gerade vor dem Wiederlernen gezeigt.

Die rückwirkende Hemmung erweist die Neigung, bei Zeitabständen von 20 Minuten, einer Stunde, 48 Stunden, und 144 Stunden, stärker zu wirken, wenn das interpolierte Lernen gerade vor dem wiederlernen stattfindet, als wenn es sofort nach dem ursprünglichen Lernen stattfindet. Die Unterschiede nach dem 24-stündigen Zeitabstand sind nicht konsequent; ein Kontrollversuch weist aber darauf hin, dass dieser Zeitabstand keine wichtige Ausnahme zu dem allgemeinen Schluss bildet. Diese Befunde widersprechen die Voraussagung, der Perseverationstheorie nach, und weisen darauf hin, dass die Perseveration höchstens eine untergeordnete Rolle bei der Bestimmung der rückwirkenden Hemmung spielt. Dr Schluss eines früheren Berichts, dass keine gleichmässige und konsequente Variirung des Prozentsatzes der rückwirkenden Hemmung mit dem Zeitabstand stattfindet, wird durch die vorliegenden Versuche bestätigt.

McGeoch

THE RELATION OF VISUAL SENSITIVITY TO THE AMOUNT OF RETINAL PIGMENTATION

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Harry Helson and J. P. Guilford

I. Introduction

The exact part played by the pigment cells in the first layer of the retina has been a matter of dispute for some time. It is generally agreed that this pigment exercises a protective function by its absorbing action and reduces the amount of reflection from the choroid coat, thereby aiding sharp vision. Statements may be found to the effect that it regenerates the visual purple of the rods, although the manner in which it does this is as yet unknown. The usual conservative view of its functions does not admit of any direct participation by the pigmentary epithelium in vision, this being reserved for the rod-pigment or visual purple and the cone-pigment. The chief objection against the retinal pigment as a photosensitive substance arises from the fact that it is practically inert chemically, although it does bleach very slowly under the action of light. On the other hand, the retinal pigment is an excellent absorbent for the whole visible spectrum, and recent speculations make much of the fact that in order for light to be effective it must be absorbed, the degree of effectiveness being related to the amount of absorption.

The importance of the retinal pigment in vision has been stressed by Zikulenko and Adler (1), who point out that the fundamental light-receiving unit cannot be regarded as the rod or cone cell alone but one of these in connection with a pigment cell. This constant connection, says Adler, is found almost universally throughout the animal kingdom. Everyone knows that detachments of the retina occur between the bacillary or rod-cone layer and the pigmentary layer and usually result in blindness, showing that the latter is essential to vision either because of its nutritive functions or because it plays an even more direct part. It is of importance, in this connection, to note that, in the human at least, the pigmentary layer with a few inner granule cells, is the only retinal layer to extend through the foveal region, besides the rod-cone layer. Johnson (13) shows that in the macula the cones are longer and normally extend into the hexagonal pigment layer. Finally, we must mention the fact that albinos lacking in retinal pigment have notoriously poor central vision and generally fail to reach as high a level of sensitivity under dark-adaptation as normals. The fact that the shape of the curve of dark-adaptation for the albino is similar to that of the normal has been erroneously interpreted as showing that the former possesses normal peripheral vision.

The evidence against the hypothesis that the retinal pigment is essential to vision comes mostly from the work on albinos and measurements of dark-adaptation. Investigators measuring the sensitivity of differently colored eyes have reported no constant relation between the amount of pigment and threshold value. Mosso (20) tested 50 subjects with differently pigmented fundi and concluded that those with more pigment were not more sensitive than less well-pigmented individuals under conditions of dark-adaptation. He found, however, a fact which is commonly overlooked, even when his results are quoted, and which is contrary to the generally accepted findings with albinos: the four albinos he tested failed to attain as high a level of sensitivity after a given period of dark-adaptation as normals. But Takagi (24) found two albinos were "normal" and concluded that pigmentation has nothing to do with dark-adaptation; similarly, Derby, Waite, and Kirk (2) reported that in seven cases of albinism tested "no material alteration of the light minimum could be made out."

Other studies with Negroes and rats in which *acuity* was tested as well as *sensitivity* seem to point to close relationship between pigmentation and vision.[1] Johnson (13) reports in the case of a Congo native boy: "The vision was supernormally acute and equal to 6/1.5 or the fourth line of Snellen's type below normal 6/6. He could see three of Jupiter's moons with the naked eye as evinced by his marking their position on paper which was confirmed by a field glass, and could spell correctly the words of the leading article of the

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[1] Many of the studies do not distinguish between visual acuity and sensitivity either in the discussion or in the conditions of experiment. It is probably true that the two are closely related and may even amount to the same thing in certain conditions, but ordinarily we must distinguish between the minimum amount of light necessary to evoke sensation (light minimum, *minimum visible,* light threshold), and the ability to perceive two stimuli as distinct (acuity, resolving power). This study is concerned with the light minimum, a matter of sensitivity.

'Times' at ten feet." Roy (23) was surprised at the excellent vision of the Negroes whom he tested in various parts of Africa. "My experiments," he says, "repeated frequently and at different places, show that the Negroes see at night from two to four times better than the whites" who were tested simultaneously. He also comments on the excessive amount of pigment in their eyes. Lashley (18), working with rats, found that pigmented rats had a threshold for striae below 52′ and above 26′, while albinos were above 52′ for the best rat and above 1° 26′ for the poorest. Robinson and Wever (22) found that albino rats could discriminate an open from a closed pathway at 18 inches, while the pigmented rats could do so only at 30 inches. When the illumination was reduced, both lost their discriminative ability, but the pigmented rats were superior throughout.

It is hardly necessary at this point to indicate how the defenders of the retinal-pigment hypothesis have met the objections raised against the primacy of the pigment for visual functions, or to point out that the opponents of this theory have failed to account for the superiority of pigmented individuals under certain conditions. The evidence seems to establish the fact that the retinal pigment is important for visual *acuity* but so far conclusive evidence for its effect upon *sensitivity* has not been furnished.

Some chance observations by Guilford (7) and Judd (14) lend support to the view that the retinal pigment is more or less directly concerned with visual sensitivity. Under conditions almost identical with those used in this study, Guilford found that the thresholds of a very small group of subjects under dark-adaptation differed extremely, the best being about five times as sensitive as the poorest. The lowest thresholds were found in two brown-eyed subjects, the highest in a pale, blue-eyed subject, and a medium threshold in a hazel-eyed subject. Judd, in his study of the Purkinje after-image under very different conditions, found among six subjects that two brunettes perceived the Purkinje image at very low intensities, two blondes required about 10 times as much illumination, and two red subjects (one a red-headed female, the other a red-bearded male) required nearly 100 times as much illumination to perceive the image. Such results, we believed, might appear significant if confirmed by a sufficiently large number of subjects.

In this study the amount of retinal pigment was inferred from the color of iris since the correlation between the melanin and iris pigments is almost one-to-one. Histologically, the pigment epithelium of the retina (melanin) belongs to the stratum pigmenti or third coat of the eye, while the pigment epithelium of the iris belongs to the tunica interna of the fourth layer. But the correlation between the two is so close that it is safe to assume, as we have done, that a dark iris will be accompanied by a dark fundus and a light iris will be accompanied by a fundus having correspondingly less pigment.

We decided, therefore, to determine if there is any correlation between sensitivity and retinal (and iris) pigmentation by measuring a sufficiently large number of subjects so as to insure statistical reliability for any differences which might appear to be correlated with pigmentation. Measurements were made not only in the peripheral retina but also in the fovea since the work on acuity has concerned foveal vision mainly while the work on sensitivity has been done in the periphery. The bearing of our findings on the relation of pigment to sensitivity will appear from the discussion of results. We believe they furnish a satisfactory basis for reconciling the positive assertions of both those who have favored and those who have opposed the theory of participation by the retinal pigment in vision, although we do not mean to imply that we have the solution for all problems concerning visual sensitivity.

II. PROCEDURE

Three groups of subjects were tested.[2] The first group consisted of 82 Bryn Mawr students having an average age of about 20 years.[3] Before the tests were made, each individual was assigned to either a light, medium, or dark category on the basis of color of iris. Any errors in classification would tend to reduce differences in sensitivity if there are differences associated with the amount of pigment and so would be unfavorable to our hypothesis, hence no objection can be raised against the fact that we did not examine the fundi of our subjects to determine the amount of pigment present. Moreover, since we wished only a threefold classification this procedure was sufficiently exact for this purpose. Of the 82 subjects, 33 were assigned to the *light* subgroup, 27 to the *medium* subgroup which included gray, green, hazel, and a few very light brown eyes, and 22 to the *dark* subgroup. The tests on this group were made only in the foveal region. This testing occupied the greater part of the school year. The following year we tested two more groups of subjects, the first consisting of 25 Negro employees of the college, ranging from 18 to 30 years of age, the second being composed of students, some of whom had acted as subjects the preceding year. The second white group was necessary because we needed a white group for measurements in the peripheral regions. The Negroes were all assigned to the dark group and will be labeled 'Negroes' in this paper although we were able to classify 9 as very dark, 9 as medium, and 7 as light. But the 7 light Negroes were darker than our dark whites and so we shall treat the Negroes as a unit.

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[2] Most of the tests were conducted by the Misses Jane Moore and Alice Yarnelle to whom we are indebted for this part of the study.

[3] Threshold values vary with age as Piper (18) has shown. If anything, the Negroes were slightly older, a fact which makes their lower thresholds the more significant.

The Negroes and the second group of whites were tested in the fovea and at 5° 55′, 10°, and 15° from the fovea. Binocular vision was used in the tests throughout all the experiments.[4]

As light source the Nagel adaptometer[5] served to provide the stimulus-light which was a very good white owing to the blue filters in the instrument designed for our very purpose. Instead of using the large front face of the instrument for stimulus, we covered it with a black cardboard having a circular aperture of 1000 sq. mm. (35.6 mm. diam.). This instrument is so arranged that the intensity of light can be varied from 1 to 8,000,000 units by means of reducing-filters, screens, and diaphragm. After the threshold has been roughly determined in a preliminary series of tests, the subject is instructed to turn a handle which increases the intensity of the stimulus until it can be *just seen*.[6] The subjects were warned not to wait until they recognized the form of the stimulus, because, as Helson and Fehrer (12) showed, the threshold for *form vision* is from 12 to 25 times higher than that for the light minimum.[7] Since it is very difficult to be sure that the subjects actually see the stimulus as they approach the limen, they were checked occasionally by blotting out the stimulus when it was reported, and, if this fact were not noted, the trial was repeated. Fifty determinations were made for each subject using only the ascending procedure to avoid form perception and for other obvious reasons.

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[4] Binocular vision is twice as sensitive as monocular, according to Messmer (19), and Piper (21).

[5] This instrument is a standard apparatus for testing rate and level of dark-adaptation and has been described and pictured in numerous places, the most available being Helmholtz (11, pp. 315ff.).

[6] The procedure we used gives a higher threshold than either of two others open to us. We might have begun with intensities above the limen and reduced the illumination to the just unnoticeable point, or we might have averaged ascending and descending series. The method we chose eliminated form perception which involves complicating factors we wished to avoid.

[7] Even otherwise careful workers have neglected this fact. Thus Hecht (10) in his study of foveal adaptation reports that he measured the light minimum in spite of the fact that he tells us that he "determined the mini-

The distance of the subjects from the stimulus was 275 cm. so that the angle subtended by both the stimulus- and the fixation-light (the latter to be described shortly) was 57′, or well within the rod-free area. In order to insure constant fixation we used as fixation-light (not to be confused with the stimulus-light) a small red cross. Only in this way is it possible to (1) test foveal fixation, and (2) be sure that one is working at any given distance from the fovea in peripheral regions. Red is the one color under conditions of dark-adaptation to which the fovea gives a greater response than any other part of the retina and hence makes fixation possible.[8] When working on foveal sensitivity the cross was so arranged as to appear to come, by reflection in a mirror, from just above the stimulus-light. When working in the periphery we merely changed the position of the fixation-light to the subjects' right. It is of special importance that the brightness of the fixation-light be kept absolutely constant owing to its inhibiting effect upon the stimulus-light (Heymans' law). Our fixation light had an area of 80 sq. mm. and was supplied with a constant current under a pressure of 2 volts. It consisted of a small automobile light placed in a screened box in which the cross was cut and covered with red filters until barely visible to the subjects.

Measurements were made after 30 minutes' dark-adaptation. This period proved sufficient to insure a fairly stable retinal equilibrium since retests on a number of individuals showed no significant differences between daylight and night thresholds. But it must be remembered that we were not measuring the rate of dark-adaptation

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mum illumination at which the subject could clearly see the object as a *cross*." Obviously the threshold for form was actually determined and not the light minimum. If his values are compared with ours they will be found to be considerably higher for this reason, although from the other conditions of his experiments we should expect them to be lower than ours.

[8] In order to maintain fixation in the dark a red stimulus must be employed in spite of the fact that its use *raises* the threshold. We found enormous differences in threshold values when the fixation light was changed only very slightly.

or working with a fully dark-adapted eye because of the presence of the fixation-light which prevented this. Our procedure was designed to insure a stable retinal state. Hecht (10) and others have shown that pupillary changes are negligible after 20 minutes' dark-adaptation, and it is well known that the curve of adaptation flattens so much after 20 minutes that it is safe to assume a stable eye after that period of adaptation, even in peripheral regions. After each observation the subjects closed their eyes, thereby allowing the eye to return to its former state so that the momentary stimulation involved in making the measurement did not introduce any artifact. At least one minute was allowed to elapse between observations.

A few words will suffice to describe the calibration of the adaptometer. It is possible to derive readings from the instrument itself which will do for relative measurements. If absolute values are desired, the adaptometer must be calibrated against a known standard. We measured the brightness of the stimulus-light at various settings of the adaptometer by means of the Macbeth illuminometer. Our readings have been so treated as to yield millilamberts for all values given in our tables when multiplied by the factor 10^{-7}. A voltmeter in the adaptometer circuit showed the fluctuations in voltage applied to the adaptometer amounted to 2%, an error to which all our readings are subject as we made no attempt to control this factor which is small in comparison with the standard deviations of the limens.

III. RESULTS

The results for the three subgroups of the first group of white subjects and for the whole group are given in Tables 1 and 2. From

TABLE 1

THE AVERAGE, STANDARD DEVIATION OF THE DISTRIBUTION ($\sigma_{dist.}$), AND STANDARD ERROR OF THE AVERAGE ($\sigma_{av.}$) FOR LIGHT, MEDIUM, AND DARK EYES IN GROUP I, AND FOR NEGROES IN GROUP II

(To convert these values to millilamberts, multiply by 10^{-7}.)

	Light	Medium	Dark	All whites	Negroes
Number	33	27	22	82	25
Average	2551.35	2406.04	1350.44	2181.32	849.16
$\sigma_{dist.}$	2782.46	2036.92	606.41	2199.39	522.49
$\sigma_{av.}$	484.75	392.01	129.29	242.96	104.50

TABLE 2

Differences between Averages, Standard Errors of Differences ($\sigma_{diff.}$), and Critical Ratios, between Light, Medium, and Dark Eyes, and between Whites and Negroes

	Light and medium	Light and dark	Medium and dark	Whites and Negroes
Diff.	145.31	1200.91	1055.59	1332.16
$\sigma_{diff.}$	412.79	501.25	623.47	264.50
Critical ratio	0.35	2.39	1.60	5.03

these tables it will be seen that there is a difference in sensitivity between the various subgroups of whites. While not all of the differences are significant statistically,[9] they are significant in themselves since they all tell the same story: With more pigmentation we find greater sensitivity. The usual critical ratio for statistical reliability, i.e., the difference divided by the standard error of the difference, is taken as 3. The value of 2.39, however, tells us that the chances are about 992 out of 1000 that the true difference between the blue-eyed and brown-eyed whites is greater than zero. Complete reliability might have been obtained had the number of cases been much larger. As they stand, the averages for the light and dark groups tell us that *it requires nearly twice as much light to attain the minimum visible for the "average" blue-eyed subject as for the "average" dark-eyed subject.* The difference between the medium and dark groups is quite large but not so reliable, while that between the light and medium groups is small enough, apart from the critical ratio, to appear insignificant were it not for the larger differences among the other groups. Some of the overlapping among groups which is in part responsible for the small significance ratio may very well be due to errors in classification of individuals and imperfect correlation between pigmentation of the iris and that of the retina. In view of these possibilities, we have good reason for suggesting, merely on the evidence furnished by our white subjects (we have not yet discussed the thresholds for the Negroes), that sensitivity is correlated with the amount of retinal pigmentation, if not directly dependent upon it.

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[9] The usual criterion of significance has been adopted in this study, viz., Diff./$\sigma_{diff.} > 3$, where $\sigma_{diff.} = (\sigma^2_{av._1} + \sigma^2_{av._2})^{1/2}$. It should be noted that the value given for the threshold of any group or subgroup is the average of the individual thresholds in that group.

Inspection of the actual results shows that often blue-eyed subjects have lower thresholds than dark-brown or black-eyed subjects. This may account for the statement found so often in the literature that pigmentation plays no part in visual sensitivity, especially if a small number of individuals have been tested. But if a sufficient number of individuals are examined and the results treated by statistical methods generally regarded as valid for phenomena which we have reason to believe obey the normal law of distribution, it will be found that, in the long run, a difference in the amount of pigmentation will be accompanied by differences in sensitivity. At this point we must remind the reader that we have so far been discussing the results for *foveal* vision only. We shall see later that as we leave the fovea the differences become smaller and smaller for the various groups (including the Negroes) until in the most sensitive part of the retina they are practically alike. This fact also accounts for the failure of previous investigators to find differences in sensitivity associated with pigmentation because most of the tests have been made under conditions of dark-adaptation without controlling fixation. The result has been that comparisons have really concerned peripheral sensitivity which we also found shows small differences for differently pigmented individuals.

In order to check our findings we next tested 25 Negroes, who are known to possess a maximum of pigment, and should, therefore, have the lowest thresholds. The results for the Negroes are also given in Tables 1 and 2. The average value for the Negroes is almost one-third that for the average for all the whites (849:2181). Our statistical criterion of significance needs hardly to be invoked in this case in view of the enormous difference, but its value, 5.03, leaves little doubt as to the significance of our findings. From statistical considerations we may say that we can be "absolutely certain" that the true difference between all the whites and Negroes is greater than zero and not due to chance.

It might well be asked if the Negroes are more sensitive than the most sensitive whites. If the data given in Tables 1 and 2 are used to calculate the reliability of the difference between the dark-eyed whites and the Negroes, the critical ratio will be found to be greater than 3. Moreover, a scatter diagram of individual results shows this to be true also. On the other hand, we find, from Tables 3 and 4

TABLE 3

Results for Control Group of Whites and Negroes in the Fovea, 5°55′, 10°, and 15°

(To convert these values to millilamberts, multiply by 10^{-7}.)

	Fovea	5°55′	10°	15°
Whites 12 cases				
Average	1473.26	426.67	148.41	138.69
$\sigma_{dist.}$	722.07	496.61	193.58	168.08
$\sigma_{av.}$	208.45	143.36	55.89	48.53
Negroes 23 cases				
Average	604.26	138.26	84.74	111.35
$\sigma_{dist.}$	478.51	134.97	63.67	128.60
$\sigma_{av.}$	97.68	27.55	12.98	26.25

TABLE 4

Actual Difference, Standard Error of the Difference ($\sigma_{diff.}$), and Diff./ $\sigma_{diff.}$ between Whites and Negroes for Foveal and Peripheral Regions of the Retina

	Fovea	5°55′	10°	15°
Difference	868.99	288.40	63.67	27.34
$\sigma_{diff.}$	230.19	145.98	57.39	55.16
Diff./$\sigma_{diff.}$	3.78	1.98	1.11	0.50

that the second group of whites has an average threshold near the subgroup of dark-eyed whites, i.e., they happen to have fairly low thresholds, yet the difference between them and the Negroes is significant. If we compare the light and medium whites with the Negroes, the superiority of the highly pigmented individuals emerges most clearly. The rough experiments of Roy and the many scattered references in the literature to the superiority of the Negro receive full confirmation here.

We had assumed that if the strongly pigmented eyes possessed superior vision in the fovea it would be quite natural that their superiority should be maintained in peripheral vision as well. The measurements were accordingly extended in the case of the second group of whites and the Negro group to the peripheral retina. Thresholds were determined in the manner described above at 5°55′, 10°, and 15° from the fovea. In Tables 3 and 4 will be found the threshold values for the whites and Negroes in these regions as well as the statistical data for determining if there is a reliable difference between the two groups. Much to our surprise and contrary to expectation the difference between whites and Negroes, which was so unmistakable in the fovea, grows smaller as we approach the periphery. While the Negroes are superior from center to periphery, a fact which is in itself significant since the difference favors the Negro throughout, the difference is not sufficiently great to be statistically reliable. Although the threshold for the Negroes at 5° 55′ is only about two-third that for the whites, it is not sufficiently lower to be significant statistically. At 15° it is negligible. We did not go beyond 15° because it is a well-known fact that sensitivity decreases from then on. One fact needs further check: Whereas the whites appear to be most sensitive at 15°, the maximum sensitivity for the Negroes seems to be at 10°, for the lowest value appears at the latter region for them while the whites do not attain their minimum threshold until 15°.

We are now in a position to explain why previous investigators have asserted that albinos have normal peripheral vision. Differences in pigmentation we have found make themselves felt chiefly in foveal vision and immediately adjacent regions, and hence measurements in the periphery alone would fail to reveal any relation between pigmentation and sensitivity. Unless special measures are taken to insure foveal vision, under conditions of dark-adaptation the periphery is bound to be the region tested, since the dark-adapted eye turns so as to bring the stimulus upon the most sensitive part of the retina. Moreover, it is impossible to secure foveal vision with

TABLE 5

ACTUAL DIFFERENCE, STANDARD ERROR OF THE DIFFERENCE ($\sigma_{diff.}$), AND DIFF./$\sigma_{diff.}$ WHEN VARIOUS PARTS OF THE RETINA ARE COMPARED WITH EACH OTHER IN WHITES AND NEGROES

	Whites			Negroes		
	Diff.	$\sigma_{diff.}$	$\frac{\text{Diff.}}{\sigma_{diff.}}$	Diff.	$\sigma_{diff.}$	$\frac{\text{Diff.}}{\sigma_{diff.}}$
Fovea and 5°55′	1046.59	249.43	4.20	466.00	101.48	4.59
Fovea and 10°	1324.84	215.81	6.14	519.52	98.53	5.27
Fovea and 15°	1334.57	214.02	6.24	492.91	101.14	4.87
5°55′ and 10°	278.26	153.86	1.81	53.53	30.47	1.76
5°55′ and 15°	287.98	151.34	1.90	26.91	38.06	0.71
10° and 15°	9.72	74.00	0.13	26.61	29.30	0.91

TABLE 6

Ratios of Foveal to Extra-foveal Thresholds of Whites and Negroes

	Whites	Negroes
Fovea and 5°55′	0.28	0.31
Fovea and 10°	0.09	0.20
Fovea and 15°	0.10	0.21

albinos whose central vision is notoriously poor or almost lacking. Since differences in pigmentation affect foveal vision most the poor central vision of albinos finds explanation in the light of our findings. Our failure to find greater differences in the periphery among differently pigmented individuals also explains the widely held belief that pigmentation has nothing to do with sensitivity.

Our experiments confirm the classical theory that under conditions of dark-adaptation the periphery of the eye is more sensitive than the foveal region. The difference between foveal and peripheral vision for both whites and Negroes is statistically reliable, as shown in Table 5. But it is interesting to note that the differences among peripheral regions when compared with each other are not statistically reliable. In other words, the great and striking difference is that between foveal and extra-foveal regions. In Table 6 will be found the ratios of sensitivity of foveal to extra-foveal regions for whites and Negroes. At 10° and 15° eccentricity the ratio is greater for the whites than for the Negroes, which is what we should expect if the whites are not as sensitive as the Negroes in the foveal regions but are nearly equal to the latter in the periphery.

We are now in a position to evaluate by way of summary the results reported in this study and their bearing on theory.

IV. Theoretical Considerations

We have found that, if enough cases are examined to insure statistical reliability, there will be found significant differences in sensitivity between strongly and less strongly pigmented eyes among white subjects. If Negroes are compared with whites the difference in sensitivity is so great as to leave no doubt on this point if foveal vision only is taken into account. While the superiority of the Negroes extends into the periphery, once the fovea is left the differences are not statistically reliable for the number of individuals examined in this study. Examination of a far greater number of cases might establish a reliable difference in favor of the more strongly pigmented subjects in extra-foveal regions as well as within the fovea.

Our findings reconcile conflicting points of view with respect to the relationship of pigmentation to sensitivity. Since we failed to establish significant differences for the peripheral retina we can understand why those who have worked with albinos under dark-adaptation without fixation-light have failed to note any correlation between pigmentation and sensitivity. Several facts serve to support the view that individuals lacking in pigment suffer from more or less reduced sensitivity in that they require more light which manages to reach the retina. It is very probable that blue-eyed individuals who lack stroma pigment and albinos who lack the retinal pigment get more light through the iris than do more strongly pigmented individuals. Thus Mosso (20) found that a diaphragm placed in front of an albino's eyes raised the threshold, whereas for the normal no difference could be detected, within certain limits. Greater transparency of iris might well compensate for lessened retinal sensitivity. Another compensating factor may be found in the size of pupil: Langfeld (17) has reported that of 7 subjects with abnormally large pupils, 3 had light blue irises, 3 medium light irises, and only one a dark brown iris. If this finding is substantiated with a larger number of subjects, it may be true to say that the non-pigmented eye requires and gets more light through its larger pupillary aperture. These two factors, pupillary size and transparent iris, may serve to equalize retinal differences in sensitivity so that under conditions of everyday life blue-eyed individuals are not at a disadvantage compared with brown-eyed.

Finally, the fact should not be overlooked that the so-called normal vision of albinos which is generally cited as crucial evidence against the retinal pigment theory is really not normal in any sense of the word. In the first place, their foveal vision is so poor as to be practically nil, and, in the second place, careful investigation of the final level of dark-adaptation reached by albinos, as shown in Mosso's work, shows that they do not reach as high a level of sensitivity as the normals. Moreover, the tendency of albinos to develop various forms of eye trouble is a well-known fact to ophthalmologists. That they can see as well as they do without the retinal pigment has been explained as follows by Adler (1): (1) all protoplasm must absorb a minimum of light and hence responds more or less to it; (2) the visual purple sensitizes the rods so that they are sensitive to low illuminations. In the light of all the evidence it seems to us that the vision of albinos furnishes no arguments against the correlation between pigmentation and sensitivity—on the contrary the albino furnishes good evidence for it.

Two possible interpretations are open to us concerning the rôle of the retinal pigment in vision, provided we leave aside for the moment the question concerning the part played by the pigment in acuity.

First, we may advance the hypothesis that the retinal pigmentary epithelium is directly affected by light and its reaction to light represents the first stage in the chain of processes leading to sensation of light. Supporting this view may be cited the *a priori* consideration that for any light to be effective it must first be absorbed and the degree of its effectiveness is a function of the amount of absorption. As Adler points out, the retinal pigment epithelium is an ideal absorbent for the whole visible spectrum. This hypothesis is further supported by the results of this study since the simplest assumption for explaining our results would be that which directly correlates differences in sensitivity with the action of the pigment. The chief objection to this hypothesis lies in the fact that the pigment cells, or rather the melanin pigment in them, are chemically inert, resisting chemical change under the action of the strongest chemical re-agents. Against this objection there can be mustered several important arguments: (1) Kühne (16) has shown that in spite of the resistance shown by fuscin to chemical action it does respond to light by bleaching, although very slowly. On the other hand, Kühne was unable to produce any change in the colors of the cone-globules by exposing the retinas of living birds to the sun for several hours, yet he pointed out that there might have been a slight change sufficient to arouse visual sensation but insufficient to be detected. May not a very slight, almost unobservable change in the pigmentary layer be responsible for vision? (2) If, as seems probable, the regenerative powers of the hexagonal pigment cells do not depend upon the fuscin they contain, then the pigmentary layer might still be directly concerned in vision through the action of some other substance in the pigment cells. This has been suggested by Halliburton when he says: "It is possible that the rods contain the precursor of visual purple, and this is acted upon by some other substance from the hexagonal cells; or it may be that the hexagonal cells withdraw the supposed substance from the rods and work it up into visual purple" (8, p. 463).

The second hypothesis which may be advanced to account for our results is less radical than the first, in the light of present knowledge, and merely reasserts the *correlation* found between amount of pigment and sensitivity without postulating that light acts directly upon the pigment in question. This might be explained by assuming that there is a correlation between amount of pigment and whatever factors are responsible for sensitivity such that whenever more pigment is present there will be found greater sensitivity. This hypothesis was suggested to us by Dr. W. T. M. Forbes of Cornell University who pointed out that a common genetic factor might be responsible for both amount of pigment and retinal sensitivity. In view of the unknown relations existing between the retinal pigmentary layer and the rod- and cone-pigments, this second hypothesis is perhaps the safer, even though it involves unknown genetic and physiological factors.

The reader has perhaps noticed that our discussion of the relations between pigment and sensitivity has so far been limited to the peripheral retina, or visual purple, although our results were statistically reliable for the fovea only. It might be urged that if the retinal pigment is important for sensitivity and in view of its necessity for the regeneration of the visual purple, why were not greater differences in sensitivity found in the peripheral regions? We have not stressed the relations of the retinal pigment to the foveal region because so little is known of the cone-pigment as compared with the rod-pigment or visual purple. In the case of the fovea we would be limited almost wholly to pure speculation. But this much can be said to explain the fact that apparently the fovea is most affected by the presence or absence of hexagonal pigment cells. If we take into consideration the relative sensitivities of foveal (photopic) and peripheral (scotopic) regions, and assume, as Forbes (3, 4) has done, that the concentrations of photosensitive substances is in the same ratio as their sensitivities, then we should expect the foveal concentration to be only 1/1000 of the extra-foveal concentration. If this is true, anything which affected the concentration in the fovea even slightly would greatly affect sensitivity, since a certain critical minimum concentration would be necessary for any vision at all. In peripheral vision where the concentration is much greater, the same change would have less effect upon sensitivity. On the basis of this explanation we should, however, expect sufficient differences in amount of pigmentation to make themselves felt in sensitivity even in peripheral vision. This implication of our theory finds support in our results wherein it was shown that the Negroes actually were superior in peripheral vision at every point tested, although the differences were not sufficiently great to be reliable for the numbers we examined. But the fact that the differences were all in the same direction, favoring the more pigmented individuals, lends weight to our position in spite of the lack of statistical reliability which might be overcome with a larger number of cases.

In concluding we should like to point out that our findings have, we believe, practical as well as theoretical importance. At present ophthalmologists and others are interested in norms of sensitivity, especially as tested by dark-adaptation. One of the difficulties encountered in establishing norms has been the wide variation in threshold values when numerous individuals were examined. From our work it would appear that there are different *levels* of sensitivity for variously pigmented eyes, apart from the question of rate of adaptation which we have not investigated. At least three classes should be distinguished in the light of present knowledge: albinos, who have the highest thresholds, normal whites, and Negroes, who possess the lowest thresholds, in foveal vision. The probabilities at present favor a further subdivision among whites into at least the classes of light and dark eyes. Sufficiently large numbers of cases will probably establish significant differences between all differently pigmented eyes, other factors held constant. The wide variation characteristic of measurements of thresholds in vision may be due in part to the factor of pigmentation. It would appear that an individual's threshold can be appraised only by reference to the pigment-group to which he belongs. The attempt to strike an average value from differently pigmented individuals in the effort to establish norms of sensitivity fails or gives a meaningless result. Only further work on hundreds of individuals under the most carefully controlled conditions can show exactly where classificatory lines must be drawn and where they may be safely neglected so far as the rôle of the retinal pigmentary layer is concerned.

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LA RELATION DE LA SENSIBILITÉ VISUELLE À LA QUANTITÉ DE PIGMENTATION RÉTINIENNE

(Résumé)

Pour déterminer s'il existe une corrélation entre la quantité de pigment mélanine et la sensibilité visuelle, on a testé un total de 94 personnes de race blanche et 25 nègres après une adaptation à l'obscurité d'une durée de trente minutes avec une lumière de fixation. Le seuil moyen pour les nègres a été le tiers de celui des blancs dans la fovea. Parmi les blancs on montre que les individus aux yeux noirs possèdent un plus bas minimum de lumière que les individus aux yeux clairs, bien que les différences entre le groupe aux yeux de couleur moyenne et les autres groupes ne soient pas assez suffisantes pour être statistiquement constantes pour le nombre des cas. En dehors de la fovea les groupes aux yeux plus pigmentés sont toujours supérieurs, quoiqu'ils cessent d'être signifiants comme la région de sensibilité maxima est atteinte. Ces résultats expliquent la faible vision centrale des albinos et leur vision périphérale apparemment normale. La discussion théorique suggère deux hypothèses pour expliquer nos résultats: (1) le pigment mélanine répond directement à la lumière ou rend sensible le pigment des bâtonnets et des cônes; (2) il n'y a aucun rapport direct entre ce pigment et la sensibilité, sauf au moyen de quelque facteur génétique par lequel nos corrélations sont établies. On considère les raisons pour et contre ces hypothèses et l'on appuie sur le fait qu'il faut encore des recherches pour décider entre elles.

Helson et Guilford

DIE BEZIEHUNG DER VISUELLEN EMPFINDUNGSFÄHIGKEIT ZUR QUANTITÄT DES NETZHAUTFARBSTOFFES

(Referat)

Um zu bestimmen, ob eine Korrelation besteht zwischen der Quantität des Melaninpigments einerseits und der visuellen Empfindungsfähigkeit anderseits, wurden in Ganzen 94 Weisse und 25 Neger, nach 30 Minuten Anpassung an die Dunkelheit, mit einem Fixierungslicht [fixation light] geprüft. Die durchschnittliche Schwelle war bei den Weissen in der Fovea drei Mal so hoch wie bei den Negern. Es wird erwiesen, dass unter den Weissen die dunkeläugigen Individuuen eine niedrigeres Lichtminimum

haben, als die helläugigen, obwohl die Unterschiede zwischen der durchschnittlichen Gruppe und den anderen Gruppen nicht gross genug sind um, in ihrer Beziehung zu der Zahl der Fälle, statistisch bedeutungsvoll zu sein. Ausserhalb der Fovea sind die stärker pigmentierten Gruppen noch immer den anderen überlegen, obwohl die Unterschiede, bei der Angrenzung an die Gegend der höchsten Empfindungsfähigkeit bei Anpassung an das Dunkel, ihre Bedeutsamkeit verlieren. Diese Befunde erklären die Schwäche der zentralen Sehkraft bei Albinos und deren scheinlich normale periphere Sehkraft. In der Besprechung werden zwei Hypothesen zur Erklärung unserer Befunde vorgeschlagen: (1) Das Melaninpigment reagiert direkt auf Licht oder sensibilisiert die Pigmente der Stäbchen und der Zäpfchen [rods and cones], (2) Es besteht keine direkte physiologische Beziehung zwischen diesem Pigment und der Empfindungsfähigkeit, ausser durch irgend eine genetische Einwirkung wodurch unsere Korrelationen establiert werden. Es werden Argumente für und gegen diese Hypothesen berücksichtigt. Es wird darauf hingewiesen, dass weitere Untersuchungen nötig sind, um eine Unterscheidung zwischen den zwei Hypothesen zu ermöglichen.

Helson und Guilford

DISINHIBITION OF THE CONDITIONED GALVANIC SKIN RESPONSE*[1][2]

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St. Clair A. Switzer

One of the most important and intriguing relationships implicit in the work of Pavlov, but one which is neither systematically elaborated nor statistically treated in any of his researches or those of his collaborators, concerns the phenomenon of disinhibition and its place in the cycle of experimental extinction and spontaneous recovery of conditioned responses. This relationship is indicated in the following quotations assembled from several studies cited by Pavlov:

1. *Experimental Extinction (or Extinctive Inhibition)*

> "We cannot . . . judge the degree of extinction by the magnitude of the reflex or its absence, since there can still be a silent extinction below the zero . . . a continued repetition of an extinguished reflex beyond the zero of the positive reflex deepens the extinction still further" (18, p. 57).

2. *Disinhibition*

> "All the considerations . . . permit us to regard the temporary restoration (by an extra stimulus) of the reflex which is in process of extinction, or which is already extinguished, as based upon the removal of an inhibitory process. We . . . describe this phenomenon as a *dis-inhibition* . . . a temporary removal of inhibition" (18, p. 66).

3. *Disinhibitory After-Effect and Its Duration*

> "In all the experiments . . . the restoration of the extinguished

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*Recommended by Clark L. Hull, accepted for publication by Carl Murchison of the Editorial Board, and received in the Editorial Office, February 17, 1933.

[1] The writer is indebted to Professor Clark L. Hull for many helpful suggestions in connection with the design, formulation, and interpretation of this investigation.

[2] For suggestions regarding standardization of the nomenclature relating to the so-called "psychogalvanic reflex," the reader is referred to articles by Landis (12, 13) and Ruckmick (20).

> reflexes lasts only for a few minutes, depending on the duration of the extra stimulus and its after-effect" (18, p. 65).

4. *Spontaneous Recovery of Original Conditioned Response*

> "All those conditioned reflexes *which have been fully established* invariably and *spontaneously return sooner or later* to their full strength" (18, p. 58) (italics ours).

Investigators of conditioned responses in *Homo sapiens*, with one or two exceptions, notably Winsor (25), have disregarded this problem of disinhibition. This neglect of a phenomenon which may be proved or disproved readily by experimental test has led to no little disagreement and confusion as to the essential characteristics of "experimental extinction." Is experimental extinction a form of inhibition (extinctive inhibition), or is it merely fatigue, or negative adaptation,[3] or a combination of negative adaptation and inhibition, or something entirely distinct from any of these?

The fact of disinhibition, if it can be established as clearly with human as with canine subjects, should tend to throw some light upon the nature of the process involved in experimental extinction. The details of the picture should be even more sharply delineated if the additional phenomenon of spontaneous recovery be investigated in conjunction with the two above-mentioned aspects of the conditioned response.

It is the purpose of this paper, therefore, to consider certain relationships found to exist (with adult human subjects and under the conditions of this experiment) among the Pavlovian principles of:

1. Experimental extinction,
2. Disinhibition,
3. Disinhibitory after-effect and its duration, and
4. Spontaneous recovery of the original conditioned reflex.

APPARATUS

In 1930, Jones (10) reported a significant study with human infants involving principles (1) and (4) above, in which he employed a galvanic-skin-response technique[4] similar to that used in the present investigation. The Jones data were obtained from measurements of the phenomenon of Féré, which consists of galvanometer deflections produced by "an increase in the current passing through the body from an external source" (24). The present data represent measurements of the phenomenon of Tarchanoff (T-phenomenon) which "is a somewhat more complex change in the current which passes between two electrodes on the skin when no external source of current is used" (24). However, as Jeffress has pointed out: "There is a high degree of relationship between the magnitude of the deflections obtained with and without an ectosomatic source of potential. The coefficient of correlation indicating the size of this relationship is of the order of .95. The two phenomena exhibit the same latent period" (9). Others have observed this same close relationship. Thus the one term "galvanic skin response" (GSR) is now widely used in referring to the phenomenon of either Féré or Tarchanoff.

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[3] Winsor (26), Razran (19), Humphrey (8), Hilgard (3), and others have suggested that negative adaptation and experimental extinction are one and the same phenomenon, a conclusion which appears likely. If these phenomena are the same, negative adaptation should manifest disinhibition. The validity of this assumption should be subject to straightforward verification by a rather simple experimental procedure, the results of which will be reported in a later paper.

In the present experiment dry silver electrodes, the size of a quarter-dollar, were attached to the back and the palm of the left hand. These were connected by flexible wires with a Leeds and Northrup D'Arsonval galvanometer having a sensitivity of 1130 megohms, a resistance of 1090 ohms, and a period of 11.8 seconds. A Type 102-L Decade Resistance Box was placed in the circuit. No external current was introduced.

The device for recording galvanometer deflections has been described elsewhere (23). In this case the galvanometer movements were reduced 4 to 1 on a kymogram.

Recording of finger retraction movements was effected by means of a mechanical connection between a small finger lever and a steel-wire recording spring. When the subject depressed this finger electrode, the celluloid tip of the steel-wire recording spring was raised. Upward pressure exerted on the finger key by a coiled spring placed beneath it caused a downward movement of the marker on the kymogram when the subject's finger moved. The subject kept this lever depressed throughout the experiment and all finger reactions, including slight tremors, were thus recorded.

A system of signal keys connected with a stimulus box in the experimental room enabled the experimenter to present visual, auditory, and electrotactual stimuli, and paired combinations of any two of these in any desired relationship, at precise intervals of time. Motor-driven switches in an adjoining room automatically activated the stimuli at regular intervals of 38.5 seconds. The interval between the components of a double stimulation was .375 second. Cellophane-tipped electromagnetic signal markers in parallel circuit with the stimulating mechanisms served as recorders.

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[4] An excellent summary of 264 recent papers on the galvanic skin response was published by Landis in 1932 (13). A summary by Landis and DeWick of the earlier literature on the subject appeared in 1929 (14).

A tetanizing faradic shock of approximately .35-second duration delivered to the pad of the middle finger of the subject's right hand was employed as the unconditioned stimulus. The more diffuse electrode was attached to the right wrist. The source of the shock was a Porter inductorium in an adjoining room. This stimulus evoked a finger retraction and a galvanic skin response.

The conditioned stimulus was a faint light of approximately .25-second duration in a field 1 inch square. This visual stimulus was produced by a 3.8-v. flashlight bulb mounted behind a black sateen screen in the front panel of the square black hood which entirely enclosed the Morris chair in which the subject sat in a semi-reclining position.[5]

The extra, or disinhibiting, stimulus was a raucous buzzer of .3-second duration which rested on a metal resonator placed immediately behind the subject's chair.

The records were measured with a Hull oscillometer (5). On this particular model, 31 oscillometer units equal 30 millimeters.

Subjects and Procedure

Following a preliminary experiment with four subjects, the form of the main investigation was outlined. The experimental group consisted of ten subjects, and the two control groups, A and B, of ten and five subjects respectively. All were male university students at various levels of academic attainment. All were naïve regarding the GSR, and 80% were unfamiliar with the principle of conditioning. The tests were conducted in a darkened room wherein a certain amount of muffled street sound entered as a minor complicating factor.[6] Care was taken to insure a comfortable position for each

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[5] Much of the apparatus here described is a modification of that used by Shipley (22).

[6] The writer was interested to note that ordinary street sounds had evidently been rendered neutral through long adaptation, only one type (automobile backfire) producing marked galvanic disturbance.

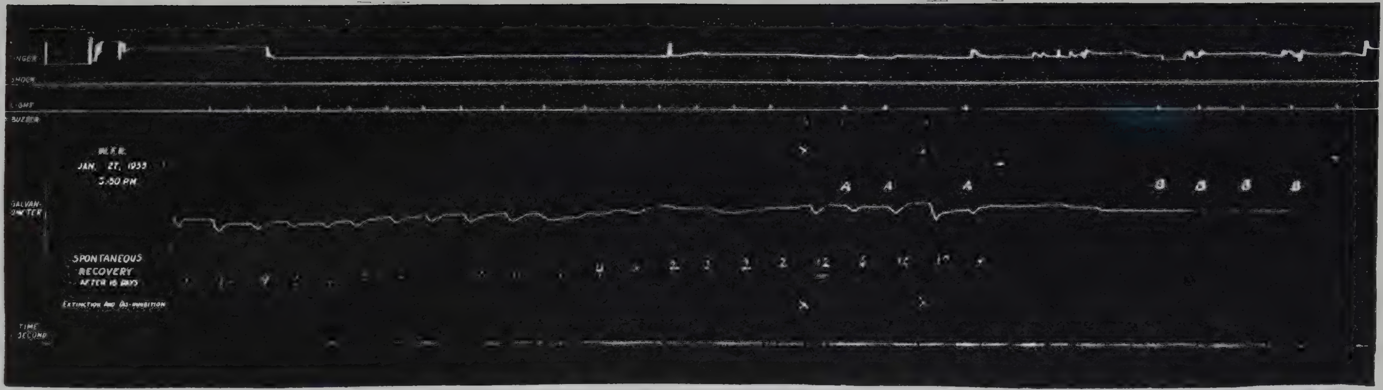

FIGURE 1

Record Showing Second Experimental Extinction, Disinhibition, and Reinstatement of Inhibition of Galvanic Skin Response (GSR) Following Spontaneous Recovery (after 16 Days) from First Experimental Extinction

(Subject IV, Table 1)

Actual length of original of above kymogram is 34 inches.

X's on the vertical lines mark points at which disinhibitor (buzzer) was introduced.

A's on galvanometer line show responses to light stimuli following disinhibition.

B's show extent of reinstatement of primary inhibition following disappearance of disinhibitory after-effect.

subject in the Morris chair. After the electrodes had been attached each subject was cautioned to keep his eyes open throughout, to make as few position-adjustment movements as possible during his occupation of the chair, and to refrain as far as possible from thinking about or attempting to control any reactions he might have a tendency to make. All subjects were unaware that any reaction other than finger retraction was being recorded. (It should be stated at this point that the only purposes served by the finger response were those of attention-arrester for the subject, and incidental check on the conditioned GSR for the experimenter.) The galvanometer, kymograph, translucent screen, and stimulus box were situated conveniently on a table slightly above and immediately behind the completely enclosed chair occupied by the subject.

The twenty-five subjects were tested for GSR to the light alone and buzzer alone at the beginning of the experiment. The purpose of these initial control tests was two-fold:

1. To measure the GSR to the light and to the buzzer as isolated stimuli at the outset, and

2. To secure evidence of any tendency toward irradiation or dynamogenesis which might produce a GSR to a neutral visual stimulus (light) 38.5 seconds after a GSR to a single application of a strong auditory stimulus (buzzer).

Following these preliminary tests the ten subjects of the experimental group were given from 13 to 38 (average 31) paired light-shock stimulations. All were tested once for a conditioned GSR to light alone after 12 such combinations. The fifteen subjects of the control groups were each given 32 isolated shock stimulations at intervals equal to those between the light-shock pairs of the experimental group. For all twenty-five subjects, occasional double intervals were introduced irregularly throughout the training series to control partially any tendency toward temporal conditioning.

On completion of the training series, the conditioned GSR's to light established in the experimental group were subjected to experimental extinction "below zero" after intervals varying from 38.5 seconds to 48 hours. The purpose of the varying intervals was to permit observation of the possible influence of negative adaptation (inhibition) caused by successive applications of the unconditioned stimulus during the training period. It seemed conceivable that this factor might show itself in the relative strengths of conditioned responses measured immediately following training and those measured sometime later when any such negative adaptation effect should have disappeared.

When experimental extinction had progressed below zero, that is, when no conditioned response to light occurred during two to three successive tests, a disinhibitor (raucous buzzer) was introduced by itself. Then (38.5 seconds later) two successive light stimuli (38.5 seconds apart) were introduced to measure the extent of revival of the extinct conditioned GSR as a result of the influence of the disinhibitor. The buzzer was again introduced, followed after 38.5 seconds by a third test light. The temporarily reinstated conditioned GSR was now allowed an interval (average 198 seconds) to relapse into its former inhibitory state, or, conversely stated, the accumulated extinctive inhibition was permitted to once again abolish the response. After this short interval of no external stimulation, four successive light stimuli were given to test the extent of reversion to the inhibitory condition.

Subsequently (3.5 hours to 16 days later),[7] the experimental group subjects were again tested with the conditioned visual stimulus to measure the extent of spontaneous recovery, without benefit of reinforcement during the interval. The entire process of extinctive inhibition, disinhibition, and reinstatement of the inhibition was then repeated. The results obtained with the experimental group indicated two essential control procedures.[8] In the case of Control Group B (five subjects) an obvious technique was employed. The program was maintained exactly as in the experimental group except that the light was never reinforced by the shock during the training series. Instead, the shock was given alone during the 32 stimulations (one more than the average number of paired combinations for the experimental group). This was followed by 19 presentations (average number required to produce extinction in the experimental group) of the light stimulus. The remainder of the tests with this group duplicated those given the experimental group. The results did not appear to necessitate a test for spontaneous recovery and this was omitted.

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[7] The interval allowed for spontaneous recovery was determined to some extent by the convenience of the subject. The longer intervals were deliberate on the part of the experimenter, however. In some instances the retest fell at hours which seemed unfavorable for the full appearance of the GSR, no doubt because of the diurnal variations noted in this response by many observers. This fact suggests that the recorded recovery is in some cases smaller than might otherwise have been the case had this factor been adequately controlled. The limited data of this experiment indicate that the hour following meals and the late afternoon hours are especially undesirable from this standpoint. Mid-morning and mid-afternoon hours gave the best results.

[8] As Dodge (1) has pointed out, the more important of the psycho-physiological factors constituting the "known conditions of variability of reaction to similar stimuli" number more than a score and a half. It is probable that the GSR is modified by all of these conditions. It was obviously desirable but practicably impossible to control all such variables in this study. It is believed that the controls herein reported were adequate for the purposes of this investigation, though far from perfect with respect to important physiological variations.

With Control Group A (ten subjects) the above procedure was altered to the extent that the buzzer was introduced after three zero or minimal GSR's to the light stimulus. This seemed to approach more nearly the essential criterion established for the experimental group. In this "A" group, three subjects gave results which seemed to warrant returning them for a retest after 24 to 26.5 hours.

Results and Discussion

The galvanometric reactions which were obtained resembled in many respects those reported by Ödegaard (17), including occasional gradual transitions from "negative" via biphasic to "positive," initial reverse deflections of small amplitude (*Vorausschlugphänomen*), and, more rarely, secondary movements following intense stimulation (*Doppelschwankungen*).

In the preliminary control tests of the light and the buzzer, five subjects showed a slight GSR and one subject a marked GSR to the visual stimulus. The strong response occurred in a subject who had been conditioned to a light in a similar experimental situation six months before. The other five had not previously been experimentally conditioned to a visual stimulus. In all six subjects this GSR to light disappeared after three to five successive stimulations.

Gesell (2) has observed that the newborn will react to light even on the first day, and the threat of a visual stimulus produces blinking in most children by sixteen weeks. These facts, when considered in connection with Jones's (10) failure to find any *galvanic* reaction to a light stimulus prior to conditioning in infants from three to eleven months of age, seem to indicate that GSR's to light stimuli, especially to those of weak intensities, are always conditioned responses or weak irradiational effects. Newhall and Sears (16) have shown that "extremely low intensities of retinal illumination are conditionable," and *a priori* expectation would lead to the hypothesis that the GSR in humans becomes associated with visual stimuli early in the life cycle.

GSR's of varying amplitude were produced in twenty-three of the twenty-five subjects (experimental and control) by the buzzer tests. In no case, however, was there clear evidence of any irradiation effect when the light was presented following this auditory stimulation.

The detailed records[9] of the responses of the experimental and control groups are given in Tables 1 and 2. The divisions of these two tables are comparable with the exception that divisions A and B are omitted from Table 2 (control groups) because the number of isolated shock stimulations was constant (32 in each case) for the fifteen subjects, and the extinction series always began 77 seconds after the final shock stimulation.

Composite graphic representations of the significant portions of these data appear in the column diagram in Figures 2 and 4. It will be noted that the mean of the first three conditioned responses to light for the subjects in the experimental group (column 2, Figure 2) is substantially weaker than that of the last three unconditioned responses to shock preceded by light (column 1, Figure 2). The fact that the members of the control groups show a small response to light after repeated shock stimulations (column 2, Figure 4) indicates that the principle of irradiation is operative, though weak.

An inspection of the values recorded for Control Group B (Table 2) would suggest that a strong irradiation tendency was manifest in the response to light in four of the subjects. However, kymograms of these subjects present a picture which is markedly different from that of the experimental group subjects. The former show movements of much longer latency and more irregular fluctuations than the latter. On the experimental group records the GSR is completed during the first half of the interval following presenta-

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[9] The record of one additional subject has been omitted. This subject gave no GSR to shock preceded by light during the training period. Tests with other subjects showed that there was no apparatus defect. Questioning elicited the information that the subject had previously been subjected to shocks under very unpleasant and somewhat similar circumstances, and also that he was undergoing intense emotional disturbance as a result of having to leave college. He was persuaded to return on the following day, when light stimuli unaccompanied by shock produced GSR's which could not be extinguished during ninety stimulations. As the subject was leaving New Haven that day it was impossible to explore further his interesting, atypical record.

TABLE 1

THE DETAILED RECORD OF THE CONDITIONED GSR'S OF THE TEN SUBJECTS OF THE EXPERIMENTAL GROUP DURING FIRST EXTINCTION, DISINHIBITION, AND REINSTATEMENT OF INHIBITION

All values in oscillometer units.

A—Number of paired light-shock stimulations in training series.
B—Interval between last unconditioned stimulus and first conditioned stimulus (light).
C—Response (in oscillometer units) to light during extinction process.
D, F, L, N—Disinhibitory stimulus (buzzer alone).
E, G, M, O—Response to light after disinhibition by buzzer.
H, P—Period of reinstatement of inhibition. Figures in parentheses represent length of interval in seconds. Figures outside parentheses show extent of galvanic movement during interval.
I, Q—Response to light after reinstatement of inhibition.
J—Interval of spontaneous recovery.
K—Response to light during second extinction process.

Order of stimulation	Subjects I	II	III	IV	V	VI	VII	VIII	IX	X
A	20	35	13	37	30	34	30	38	38	36
B	38.5 S.	10 M.	38.5 S.	48 H.	38.5 S.	10 M.	38.5 S.	6 H.	15 M.	77 S.
C– 1	58	21	37	14	48	26	11	25	5	18
2	52	6	36	36	63	27	8	24	3	7
3	60	10	30	44	25	21	16	10	3	2
4	52	8	25	55	37	6	12	5	1	1
5	50	6	34	46	29	8	5	9	0	3
6	42	8	26	54	31	8	7	14	0	0
7	60	15	19	36	18	15	6	7	1	0
8	90	15	33	32	14	39	2	0	0	
9	95	15	28	24	21	17	2	1		
10	69	11	30	53	22	8	1	1		
11	85	9	26	43	11	5	2	0		
12	121	7	13	20	4	2	2			

TABLE 1 (*continued*)

Order of stimulation	Subjects I	II	III	IV	V	VI	VII	VIII	IX	X
13	71	9	21	14	3	2	0			
14	38	6	31	34	23	2				
15	30	7	10	20	6	1				
16	22	4	11	17	3	1				
17	37	2	11	5	1	3				
18	13	4	11	1	0	0				
19	39	6	6	5	0					
20	38	5	15	5						
21	42	1	17	2						
22	14	3	9	2						
23	28	1	4	3						
24	25	2	4							
25	23	3	6							
26	25	4	3							
27	21	4	4							
28	13	2	8							
29	15	4	5							
30	8	0	0							
31	10									
32	9									
33	0									
34	2									
D–buzzer	69	19	73	64	33	35	13	26	4	19
E– 1	38	15	25	40	32	15	4	13	2	8
2	29	7	12	18	13	5	–	8	–	–
F–buzzer	68	11	23	33	58	31	9	24	7	14

TABLE 1 (*continued*)

Order of stimulation	Subjects I	II	III	IV	V	VI	VII	VIII	IX	X
G– 3	31	9	13	11	41	15	9	17	11	7
H–no stimulus	9 (231 s.)	19 (192 s.)	14 (192 s.)	3 (231 s.)	7 (192 s.)	5 (192 s.)	4 (192 s.)	37 (270 s.)	2 (154 s.)	0 (154 s.)
I– 1	9	1	0	2	4	0	1	4	1	0
2	6	1	2	4	3	5	0	1	0	0
3	3	0	2	2	2	3	1	1	0	0
4	2	0	1	4	0	2	0	3	1	0
Second extinction and disinhibition of conditioned GSR's of Experimental Group following spontaneous recovery.										
J	25 H.	17.5 H.	17.5 H.	16 D.	25.5 H.	27 H.	76 H.	16.5 H.	3.5 H.	25 H.
K– 1	16	6	11	11	4	3	4	23	4	6
2	12	6	7	13	6	2	6	11	5	4
3	5	10	4	9	4	3	5	10	6	3
4	10	7	4	6	5	4	9	23	3	1
5	2	6	0	6	0	1	12	21	1	6
6	3	6	1	8	1	1	5	7	2	1
7	1	3	1	9	0	0	7	4	0	3
8	1	5	0	8		0	2	1	0	2
9	0	3		10			2	3	0	3
10		3		11			0	4		0
11		3		6						0
12		2		4						0
13		0		4						
14		1		2						
15		2		3						

TABLE 1 (*continued*)

Order of stimulation	Subjects I	II	III	IV	V	VI	VII	VIII	IX	X
16				2						
17				2						
L–buzzer	18	10	15	12	9	11	7	33	6	7
M– 1	11	5	12	6	3	4	5	22	5	4
2	4	–	3	10	–	–	–	5	–	–
N–buzzer	55	6	13	17	8	11	5	20	–	8
O– 3	14	5	11	8	5	5	4	18	–	5
P–no stimulus	7 (231 s.)	2 (154 s.)	14 (270 s.)	5 (154 s.)	0 (154 s.)	2 (192 s.)	0 (154 s.)	23 (231 s.)	1 (192 s.)	0 (116 s.)
Q– 1	3	2	0	0	0	0	0	8	0	0
2	3	1	0	0	0	0	0	10	0	1
3	0	3	0	1	0	0	1	5	0	0
4	2	0	0	1	0	0	0	2	0	0

TABLE 2

Detailed Record of Control Groups A and B

See legend for Table 1. All subjects in the control groups received 32 unconditioned stimulations (shock alone) before being tested with the light. The test series began 77 seconds after the last unconditioned stimulus in every case. All values represent oscillometer units, and all not otherwise designated are in response to light alone.

Order of stimulation	Group A Subjects										Group B Subjects				
	I	II	III	IV	V	VI	VII	VIII	IX	X	I	II	III	IV	V
C– 1	10	3	4	3	2	1	1	1	0	3	8	2	12	0	4
2	1	8	1	5	1	0	0	2	0	1	11	1	11	0	4
3	17	3	6	4	4	0	1	2	0	0	1	4	3	1	0
4	7	0	8				1				2	4	6	0	1
5	1	0	1								3	1	1	0	3
6	3		0								7	0	0	1	0
7	0		0								9	0	3	0	5
8	1										6	3	2	1	1
9	1										2	0	1	1	4
10	5										2	1	0	2	3
11	0										9	1	0	0	0
12	2										10	3	0	0	5
13	1										1	0	1	0	3
14	0										2	0	3	0	2
15											1	1	1	0	0
16											1	0	0	0	0
17											6	0	0	0	1
18											2	2	10	1	0
19											1	0	2	0	0
D–buzzer	30	22	23	20	22	30	28	15	19	15	17	31	31	16	17
E– 1	10	4	3	1	6	0	1	1	0	1	0	0	0	0	0
2	7	0	6	2	3	1	1	0	1	0	0	1	2	1	0
F–buzzer	15	23	30	14	19	42	18	14	20	17	15	26	35	14	21
G– 3	0	2	5	3	0	1	1	0	1	1	1	0	1	0	0

TABLE 2 (*continued*)

Order of stimulation	Group A Subjects										Group B Subjects				
	I	II	III	IV	V	VI	VII	VIII	IX	X	I	II	III	IV	V
H–no stimulus	0 (192 s.)	15 (192 s.)	13 (154 s.)	22 (192 s.)	8 (154 s.)	0 (77 s.)	3 (116 s.)	4 (154 s.)	1 (116 s.)	5 (192 s.)	2 (154 s.)	1 (154 s.)	5 (192 s.)	1 (154 s.)	4 (154 s.)
I– 1	0	0	2	2	0	0	0	1	2	0	3	1	1	1	3
2	0	1	1	0	1	2	2	2	0	0	1	2	1	0	2
3	0	1	2	1	2	1	1	1	1	2	2	1	0	0	1
4	0	0	0	0	0	0	2	2	0	1	2	0	2	1	0
J–Spon. recovery	26½ (hours)	24	25												
K– 1	0	1	1												
2	0	0	1												
3	0	0	0												
L–buzzer	9	13	28												
M– 1	2	0	1												
2	0	0	2												
N–buzzer	13	24	26												
O– 3	0	0	0												
P–no stimulus	0 (154 s.)	2 (154 s.)	10 (154 s.)												
Q– 1	0	0	2												
2	0	1	0												
3	0	1	1												
4	0	1	2												

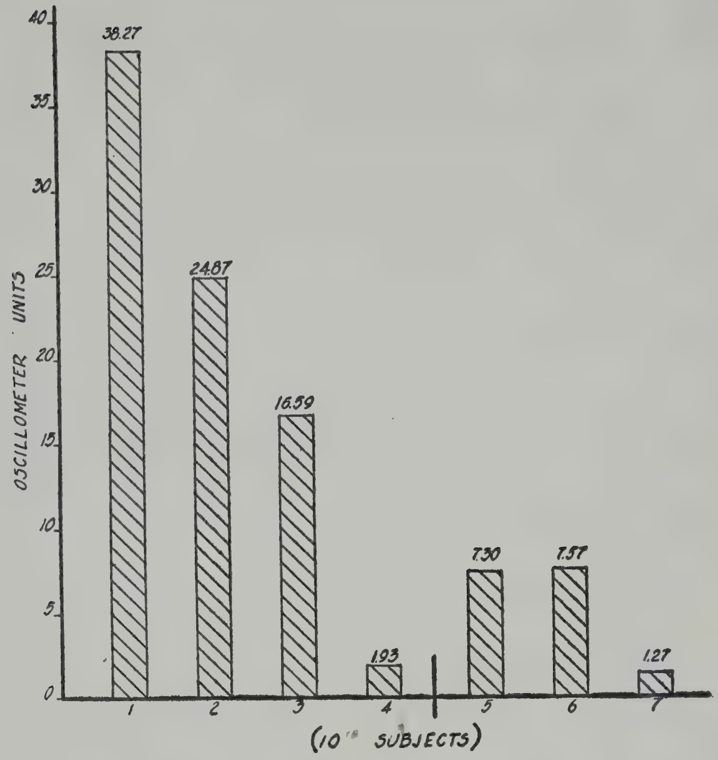

FIGURE 2

Composite Column Diagram Showing Mean GSR of Experimental Group at Various Stages in Experimental Procedure

1. Mean of last three unconditioned responses to shock preceded by light.
2. Mean of first three conditioned responses to light.
3. Mean of first three responses to light after disinhibition by buzzer following experimental extinction (27 tests).
4. Mean of first three responses to light following average delay of 198 seconds.
5. Mean of first three conditioned responses to light after spontaneous recovery periods varying from 3.5 hours to 16 days.
6. Mean of first three conditioned responses to light after disinhibition following second experimental extinction (23 tests).
7. Mean of first three responses to light following average delay of 184 seconds.

tion of the light, that is, within the first 19 seconds, with almost invariable regularity. Furthermore, no sudden changes of direction appear in the movements. On the other hand, the galvanic deflections of Control Group B appear as unpredictable movements in one direction or the other from the base line, or as a prolonged uninterrupted drift in a single direction. It was felt, therefore, that a comparison of the movements during the first halves of the intervals with those during the second halves, for Group B, would give a fair measure of the specificity of the irradiation to light. Appropriate measurements gave a value of 115 oscillometer units for the 95 anterior halves of the intervals between the 19 initial light stim-

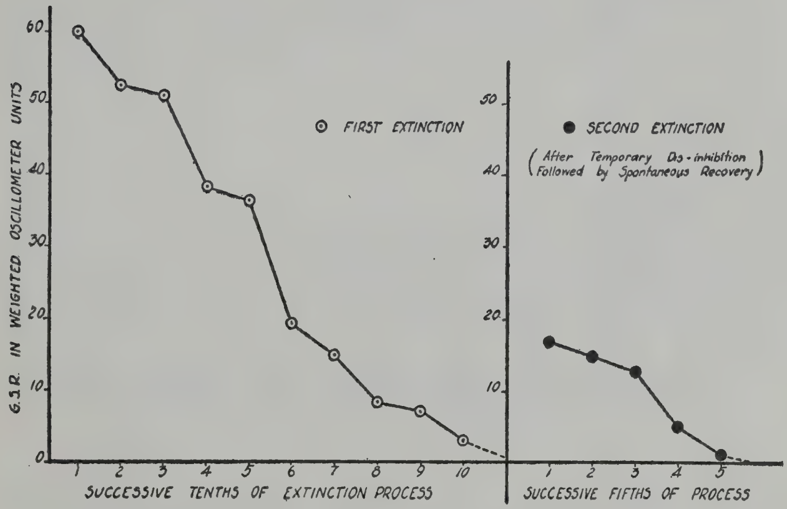

FIGURE 3

Composite Vincent Curves of Extinction Process

The first curve represents a composite of the process of experimental extinction of the conditioned GSR to a visual stimulus for the 10 subjects of the experimental group. The extinction procedure began at intervals varying from 38.5 seconds to 48 hours following the original training series (see Table 1). This extinction was followed immediately by a temporary disinhibition by an auditory stimulus.

The second curve shows a composite of the extinction process for the same 10 subjects after spontaneous recovery from the reinstatement of the primary extinctive inhibition. This second experimental extinction began at intervals varying from 3.5 hours to 16 days after the original inhibition had been reinstated following a temporary disinhibition (see Table 1 and Figure 2).

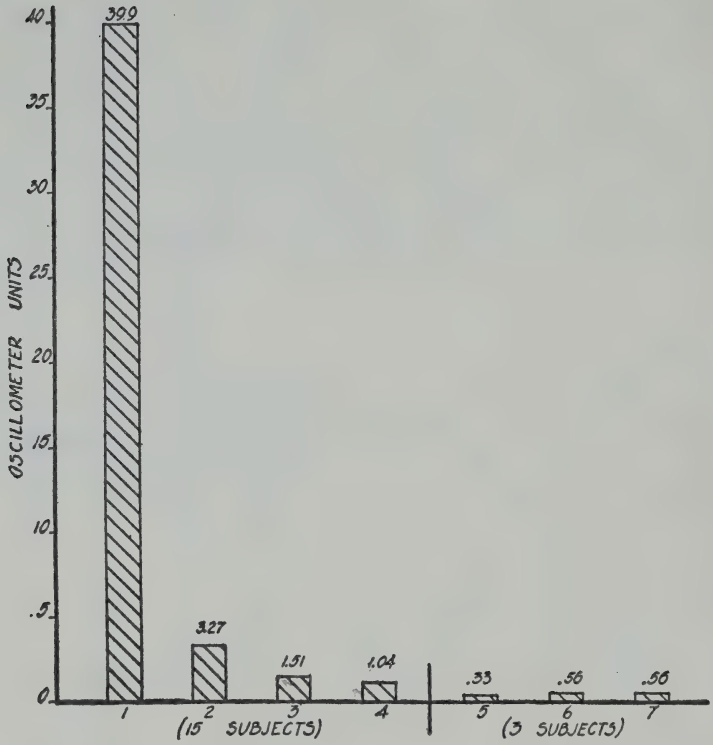

FIGURE 4

Composite Column Diagram Showing Mean GSR of Control Groups A and B (15 Subjects) at Various Stages in Experimental Procedure

1. Mean of last three responses to shock alone.
2. Mean of first three responses to light alone.
3. Mean of first three responses to light after buzzer.
4. Mean of first three responses to light after delay of 154 seconds.
5. Mean of first three responses to light after periods varying from 24 to 26.5 hours (three subjects).
6. Mean of first three responses to light after buzzer (second day—three subjects).
7. Mean of first three responses to light following delay of 154 seconds (second day—three subjects).

ulations for each subject in Group B, and 91 oscillometer units for the 95 posterior halves of the intervals. This suggests a generalized, non-specific irradiation effect which is quite distinct from the specific conditioned character of the experimental GSR's to light.

The extent of the disinhibition produced by the buzzer following experimental extinction is apparent when one compares the values in column 3 of Figures 2 and 4. The temporary nature of the disinhibitory after-effect is demonstrated by the small values in column 4 of Figure 2.

Spontaneous recovery is shown by the values in column 5, Figure 2, and by the smaller of the composite curves[10] in Figure 3. Due to the fact that three subjects in Control Group A showed small GSR's to the light following the disinhibitory stimulus, they were retested for recovery on the following day. If the irradiation effect were of sufficient strength, it might be assumed that it would present a picture similar to, though less perfect than, that of the specific conditioned response. In such a case we should expect it to suffer extinctive inhibition, to respond to a disinhibitor, to show secondary relapse into the inhibitory state, and, finally, to undergo some measure of spontaneous recovery. These phenomena were not clearly demonstrated, however, with the three subjects tested. To be sure, experimental extinction and disinhibition and reinstatement of inhibition seemed to be evidenced, but the cycle was not completed by subsequent spontaneous recovery. These data are not conclusive, and a theoretical extension of conditioned response principles demands further experimental treatment of this hypothesis.

Pavlov has said that fully established conditioned reflexes invariably and spontaneously return sooner or later to their full strength. The second curve in Figure 3 does not support this conclusion. This is not a limitation on the Pavlovian dictum, but is probably the result

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[10] The composite Vincent curves in Figure 3 were secured by weighting the values of the original Vincent curves by the following formula suggested by Professor Hull:

$$\frac{MFT}{ft} = W,$$

in which MFT equals the mean of the first tenth of the individual Vincent curves for the ten subjects, ft equals the first tenth of the Vincent curve of any subject, and W equals the obtained weight assigned to all values of the individual Vincent curve represented by ft.

This formula eliminated curve artifacts caused by the excessively large GSR's of one subject, and also insured comparability between the first and second curve.

of a failure in this experiment to meet the definite criterion of "full establishment" and the decidedly elusive one of "sooner or later."

The results of this experiment, when related to other recent investigations of conditioned response phenomena, pose several experimental problems of some theoretical significance. One of these has been suggested above in the discussion of irradiation. Others grow out of the following considerations:

1. The extinction curves in this experiment do not exhibit the initial rise which has been observed in similar curves by Switzer (23), Scott (21), Hudgins (4), Jones (11), and others. This problem merits further experimental study.

2. It is reported that Minut-Sorokhtina[11] has assumed that extinction of the basal reflex occurs because a light stimulus, instead of serving as an associative excitant, becomes an associative inhibitor to the electrodermal stimulation. If the writer understands this conclusion, it means simply that Minut-Sorokhtina is attaching a new and somewhat doubtful interpretation to one of the most frequently observed phenomena of conditioning, namely, the tendency of a previously positive stimulus to acquire negative or inhibitory characteristics as a result of lack of reinforcement. [Cf. Pavlov (18), Hull (6, 7), Winsor (25), *et al.*]

3. Pavlov has suggested the possibility that disinhibition and reinforcement are related phenomena (18, pp. 389 ff.). The nature of this relationship awaits further experimental investigation.

4. With Subject II of the experimental group, each of the 30 presentations of the conditioned visual stimulus (light) evoked complete finger retraction, no evidence of experimental extinction of this reaction being manifest up to the point of complete extinction of the conditioned GSR. The disinhibitory buzzer and all subsequent light stimuli likewise evoked this finger reaction, and it was found to be present at its full strength after a period of 17.5 hours. This fairly stable conditioning of a voluntary response (finger retraction), in connection with a more or less non-voluntary response (GSR) which coincidentally manifested typical experimental extinction, suggests that the permanence of the former (finger retraction) was due to "symbolic reinforcement." Further experimentation is indicated.

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[11] In an article in Russian not available to the writer.

SUMMARY

1. A conditioned galvanic skin response (GSR) to a previously neutral visual stimulus (faint light) was established in the ten subjects of the experimental group. A tetanizing faradic shock was the unconditioned stimulus.

2. The conditioned GSR was subjected to experimental extinction "below zero," after which it was *disinhibited* by an extra auditory stimulus in the form of a raucous buzzer. This extinguished conditioned response to the light stimulus reappeared at approximately 2/3 of its original strength after disinhibition by the buzzer. These data appear to corroborate Pavlov's conclusion that disinhibition is a genuine phenomenon.

3. The temporary after-effect of the disinhibition was demonstrated by a reinstatement of the inhibition after an average delay of 198 seconds.

4. Spontaneous recovery was evidenced after intervals of 3.5 hours to 16 days with no intervening reinforcement. The cycle of extinction, disinhibition, and reinstatement of inhibition was then repeated.

5. Preliminary tests with the buzzer and the light at an interval of 38.5 seconds failed to show distinct irradiation from the auditory to the visual stimulus.

6. Control Groups A and B (ten and five subjects respectively) were given shock stimulations, unaccompanied by light, equal in temporal order and average number to the paired light-shock stimulations of the experimental group. Sufficient irradiation and disinhibition effects were evidenced in three subjects of Control Group A to warrant a retest for spontaneous recovery on the following day. No spontaneous recovery of the irradiation tendency was discovered.

7. The curves of experimental extinction of the conditioned GSR did not exhibit the initial rise which has frequently been noted in extinction curves by previous investigators, including the writer.

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LA DIS-INHIBITION DE LA RÉPONSE GALVANIQUE CONDITIONNELLE DE LA PEAU

(Résumé)

On a établi chez dix sujets une réponse galvanique conditionnelle de la peau à un stimulus visuel auparavant neutre (lumière faible). Le stimulus non conditionnel a été un choc faradique au doigt. La réponse conditionnelle a été expérimentalement éteinte "au-dessous de zéro" et dis-inhibée par un stimulus auditif (sonnette électrique rauque). La dis-inhibition a réveillé la réponse éteinte aux deux tiers de sa première force.

La nature temporaire de la dis-inhibition a été montrée par le rétablissement de la première inhibition après un retard de 198 secondes. Un rétablissement spontané s'est montré après des intervalles de 3,5 heures à 16 jours avec nul renforcement intermédiaire.

Les Groupes de Contrôle A et B (dix et cinq sujets respectivement) ont subi des chocs, non accompagnés de lumière, égaux en ordre temporel et en nombre moyen aux stimulations de lumière et de choc en paires subies par le groupe expérimental. Trois sujets dans le groupe de contrôle A ont montré assez d'effets d'irradiation et des dis-inhibition pour justifier qu'on les teste pour le rétablissement spontané après vingt-quatre heures. On n'a trouvé nul rétablissement spontané de l'irradiation faible.

Les courbes de l'extinction expérimentale de la réponse galvanique conditionnelle de la peau n'ont pas montré l'élévation initiale qu'on a fréquemment notée dans les courbes d'extinction.

Dans la discussion on suggère plusieurs problèmes expérimentaux et certaines implications théoriques des résultats de l'étude.

Switzer

DIE ENTHEMMUNG DER BEDINGTEN GALVANISCHEN HAUTREAKTION

(Referat)

Eine bedingte galvanische Hautreaktion auf einen vorher neutral gewesenen visuellen Reiz (mattes Licht) wurde in zehn Versuchspersonen etabliert. Als unbedingter Reiz diente ein durch faradischen Strom bedingter Schock im Finger. Die bedingte Reaktion wurde "unter Nul" ["below zero"] experimentell erlöscht und durch einen auditiven Reiz (einen rauhen Schall) [raucous buzzer] enthemmt. Durch die Enthemmung wurde die erlöschte Reaktion bis zu Zwei-drittel ihrer uhrsprünglichen Stärke wieder belebt.

Die zeitweilige Dauer der Enthemmung wurde durch die Wiedereinführung der ursprünglichen Hemmung nach einem 198-sekundigen Aufschub

erwiesen. Spontane Erholung zeigte sich nach Zeitabständen von 3.5 bis 16 Tagen, ohne Einschaltung einer Verstärkung.

Die Kontrollgruppen A und B (bestehend respektiv aus 10 und 5 Versuchspersonen) erhielten Schocks, die nicht von Lichtreizen begleitet wurden, und die, in ihrer zeitlichen Anordnung und durchschnittlichen Zahl, gepaarten Licht-Schock Reizen glichen, die der experimentellen Gruppe gegeben wurden. Drei Versuchspersonen aus der Kontrollgruppe A erwiesen Einwirkungen der Strahlung und der Enthemmung die zur Rechtfertigung einer Prüfung der spontanen Erholung nach 24 Stunden genügten. Es zeigte sich kein spontane Wiedergewinnung der matten Strahlung [faint irradiation].

Kurven darstellungen der experimentellen Erlöschung der bedingten galvanischen Hautreaktion zeigten nicht den anfänglichen Aufschwung der oft in Erlöschungskurven beobachtet worden ist.

In der Besprechung wird auf mehrere experimentelle Aufgaben und auf verschiedene theoretische Anweisungen aus den Befunden der Untersuchung hingewiesen.

SWITZER

CERTAIN EMOTIONAL FACTORS IN LEARNING AND EFFICIENCY*[1]

From the Psychological Laboratory of the University of Chicago

Fred McKinney

Introduction

Frequently, in learning experiments, investigators find subjects who seem quite disturbed emotionally, either because they are being timed and feel that they are not working rapidly enough, or because they fear that their self-recognized lack of ability in intellectual tasks is being displayed. In this experiment it was intended to produce artificially, in all subjects, such an emotionally disturbed condition as occurs under many experimental conditions where ability is being measured. By this procedure the effect of such a variable on learning and efficiency can be measured. No experimental work has been done to touch directly upon this problem; the closest approach is found in the following studies:

Myers (6) compared records of two groups of 13 girls in learning four five-word lists. One group was instructed to use all the time needed to learn correctly and to learn as quickly as possible; the other was given a 9-minute time limit which was sufficient for learning. The average for the first group was 14 minutes, 47 seconds. There was practically no difference in accuracy. Awareness of a time limit seems to improve output by increasing speed, but hardly to affect accuracy.

Bills and Brown (1), in a study on the "quantitative set," used a time set as well as an amount set. Under the time set, amount was controlled by always giving the subjects enough to keep them busy throughout the period. They were instructed to work as fast as possible, but no requirement was made as to the amount of work to be accomplished. The conditions involved 2-, 4-, 8-, and 16-min-

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[1] The author is indebted to Dr. A. G. Bills for criticism and suggestions.

ute periods, and a signal was given at each quarter minute. Whereas a clear relationship was demonstrated between the initial level and decrement in the amount set, no such relationship appeared when time set alone was used. The decrements under the time set were equal for all four conditions.

Gilchrist (3) reports a 79% improvement on the Courtis English Test in the case of a group of 29 college students after praise, while 21 in a reproved group deteriorated. In this second group, students doing well on the first test deteriorated while poor students improved. Gates and Rissland (2), using motor coordination and color-naming with 74 college students, found the rank order of improvement in the former test to be: (1) encouraged group; (2) discouraged; (3) repetition (the test being repeated in this group). In the latter test the groups took the order: (1) discouraged; (2) encouraged; (3) repetition. Hurlock (4) demonstrated that on the whole praise and reproof are of equal value as incentives. Using as her material a group intelligence test, she compared 408 students of different ages, sexes, races, and initial abilities in three groups: praised, reproved, and control.

There have been numerous studies on the retention of affectively toned material. Meltzer (5) has reviewed these studies and has criticized them mainly on the grounds of unreliability and artificiality of conditions. Many investigators used too few subjects; some had unreliable questionnaires; some used conditions which were not at all true to life and, in most studies, it is doubtful whether or not a genuine emotion was induced in the subjects. None of these studies deals with an experimentally produced emotion.

Experiments [reviewed by Ruch (7) in his study of the same problem] have been conducted with intelligence tests in which the results on a test with a time limit have been compared with those on the same test with indefinite or doubled time. The correlations between a test with a time limit and the same with unlimited time are around .95. It must be remembered, however, that in such cases the subjects are told that they are not expected to complete all the items on the test, nor are they expected to have them all correct.

Precisely, the object of this experiment is to ascertain the effects of an unreasonable time limit and a suggestion of intellectual inferiority on learning and efficiency. It, therefore, differs from Myers' set-up in that the subjects were given an unreasonable time limit; it differs from Bills' and Brown's in that both a time and an amount limit were simultaneously imposed. It differs from the intelligence-test time-limit studies in that subjects were told to perform until they had reached a criterion of perfection. Finally, it differs from the praise-and-blame experiments in many respects, especially in that it was given to individuals under carefully controlled laboratory conditions and not in classroom groups as many of the latter were given, in that a time factor was involved, and in that inferiority was inferred rather than stated.

Methods, Apparatus, and Subjects

The three *conditions* under which the subjects worked were as follows:

1. *Control.* The subjects were given the instructions concerning the task, nothing being said about time.

2. *First Experimental.* The subjects were given the same instruction with the additional information, "At the end of each minute an automatic clock will buzz." The only difference in the experimental conditions was the buzzing of this clock.

3. *Second Experimental.* In addition to the information concerning the periodic buzz the subjects were told by printed instructions: "You should not take over 6 minutes to master this task. This is sufficient time for a person of average intelligence. At the lapse of this period an interval timer will ring. Should it happen that you have not finished by then, go ahead until you have finished." This experimental condition differed from the second in that an interval timer was placed on the desk to ring at the end of 6 minutes.

The *tasks* consisted of:

1. *Motor Learning.* A Chicago stylus maze[2] was used. The subjects were given printed instructions explaining that retracing of the true path and entrances into blind alleys would be counted as errors. The two types of errors were recorded separately. They were told that two perfect consecutive runs were considered as mastery and that their styluses would be removed from the maze when they had reached the end. Retention was tested after 24 hours by relearning.

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[2] A diagram of this maze will be found labeled as Maze B on page 22 in L. W. Webb's "Transfer of Training and Retroaction" (8).

2. *Ideational Learning.* A Hull automatic electric memory drum was used, giving a 2-second exposure. The list consisted of 12 nonsense syllables which were to be learned by the anticipation method, the syllables being spelled. The subjects were given typed instructions explaining what they were to do and explaining that the criterion for mastery was two perfect consecutive recitations. Retention was tested after 24 hours by relearning. And as in the maze learning, if the subject did not return at this time, his name was dropped from the relearning list.

3. *Motor Work.* A ruler-type steadiness tester was used with a small buzzer attached which would sound each time the subject made a contact. The subjects were instructed to move the stylus along the alley between the rulers to a certain point and back to the starting-point 50 times. They were told that whenever they made a contact they were to return to the starting-point and begin on another run, since all runs in which there were contacts would not be credited toward the 50 which they must complete. In addition, subjects were instructed as to how to hold the stylus and were given 5 errorless practice runs. The experimenter recorded the time for each run and all errors.

4. *Mental Work.* Twenty-five multiplication problems were given to the subjects. They consisted of three-place by three-place and four-place by four-place numbers. These were typed on cards, and the subject put the cards over his sheet of paper and did the actual multiplying on the blank sheets without copying the problem. The experimenter recorded the time taken to finish each problem, and also placed a mark with a colored pencil on the subject's paper at the end of each minute so that later the number of multiplications done per minute could be calculated.

In most cases the tasks were given on four successive days, the order of presentation being systematically varied in counterbalanced practice order, to correct any effect due to position of the task and also to ascertain whether the effect of the experimental conditions tended to change during the four days.

A one-way vision screen, permitting vision in only one direction and separating the subject and the experimenter, allowed the subject to be observed without being aware of it. This also prevented the subject from being influenced by the experimenter's behavior. Observations of the subject's facial expressions, movements, and verbalizations were made during the course of the experimentation.

The subjects were later told that they had been observed through the screen and none of them objected to this procedure.

The *subjects* were naïve as to the purpose of the experiment, and an attempt was made to choose subjects who were not accustomed to psychological laboratory situations. Data are available for 137 subjects divided equally among the three conditions. If some of the experimental sittings of a subject were ruined by accidental occurrences the data from them were discarded, but the other data from that subject were retained. Thus the number of subjects for each test under each condition varied from 40 by a small number. The majority of the subjects were the typical ones used in the Chicago laboratory, introductory students in psychology.

At the end of the four periods the subject was handed a card asking him to write what "went on in his mind" during the experimentation. No suggestions were given as to what he should answer. After he had done this he was given another card asking him specifically if anything distracted him, if he had any feelings of inferiority, disgust, etc. These papers were examined later and the frequency of the appearance of different statements was tabulated for the three conditions.

RESULTS

The data show the following facts:

1. In learning and work, *errors* show a significant increase from Condition 1 to 3 in all cases, and from Condition 1 to 2 in the case of the maze and multiplication (Table 1). Errors show a significant increase from Condition 2 to 3 in the case of multiplication. Errors show a consistent increase in all measures of those tests in comparing Conditions 1 and 2 and Conditions 2 and 3. The differences between the means divided by the standard errors, except for the significant differences mentioned above, range from .70 to 2.32. These facts seem to point quite conclusively to the generalization that errors increase rather markedly when a time factor is introduced and increase still more when a time limit and a suggestion of inferiority are introduced.

2. In no tests of learning and work was the increase or decrease in *time* statistically significant. In maze and multiplication tests the time increased from Condition 1 to 2 but decreased from Condition 2 to 3, becoming slightly less than that of Condition 1 in the case of multiplication. In only two cases out of six was there a decrease in

TABLE 1

MEANS AND RELIABILITY* MEASURES OF VARIOUS TESTS

Conditions	Means 1	2	3	Differences 3-2	3-1	2-1	Standard error of the difference 2-3	1-3	1-2	Difference / Standard error of diff. 2-3	1-3	1-2
Maze (learning)												
Errors (culs)	53.02	120.40	135.69	15.29	82.67	67.38	21.65	17.73	18.71	.70	4.66†	3.60
Errors (retraces)	39.95	78.47	103.25	24.78	63.30	38.52	15.62	12.54	12.45	1.59	5.05†	3.09
Time (in sec.)	1189.84	1655.00	1552.40	—102.60	362.56	465.16	229.60	129.70	207.10	.45	1.82	2.24
Trials	24.76	27.35	31.60	4.25	6.84	2.59	4.00	3.83	4.71	1.06	1.79	.54
Maze (relearning)												
Errors (culs)	8.16	8.55	6.98	—1.57	—1.18	.39						
Errors (retraces)	2.83	3.83	4.74	.91	1.91	1.00						
Time (in sec.)	144.53	228.09	175.38	—52.71	30.85	83.56	49.45	39.87	48.34	1.06	.77	1.72
Trials	7.03	7.26	7.17	—.09	.14	.23						
Nonsense Syllables												
Learning	47.83	43.52	53.97	10.45	6.14	—4.31	5.90	5.66	5.86	1.77	1.09	.73
Relearning	7.37	5.10	6.17	1.07	—1.20	—2.27	7.54		2.09			1.08
Multiplication												
Time	1414.25	1439.00	1327.12	—111.88	—87.13	24.75	89.81	73.94	81.95	1.24	1.18	.28
Errors	193.00	203.72	221.91	18.19	28.91	10.72	3.11	2.83	2.70	5.84†	10.21†	3.97†
Steadiness												
Time	592.10	510.31	648.63	138.32	56.53	—81.79	65.61	53.60	21.73	2.21	1.06	3.70
Errors	36.73	43.52	76.59	33.07	39.86	6.79	14.25	14.14	6.46	2.32	2.82	1.02

*Reliability was not calculated for those differences which seemed negligible.

†Statistically significant.

going from Condition 1 to the experimental conditions, and these cases occurred in the work tasks. For one of the increases in time the difference of the mean divided by the standard error of the mean was equal to 2.24, for another 2.21. These facts all seem to lead to the generalization that time may increase or decrease as the result of an introduction of a time factor into the experimental situation, but the tendency seems to be more in the direction of an increase. This is interesting in view of the fact that one would expect, on the basis of Myers' findings, that when a subject is made aware of the time element his time would decrease, time thus acting as a motivator. But apparently that is true only under some conditions and not when the limit is too short or when a clock is ringing each minute.

3. In the case of the maze, *trials* increased from Conditions 1 to 2 and from 2 to 3. In the case of nonsense syllables the trials decreased from Conditions 1 to 2 but increased from 2 to 3. None of these differences was statistically significant. This leads to the generalization that the number of trials required to learn is affected slightly by a time factor or a feeling of inferiority and more often in the direction of an increase. This slight influence of the experimental conditions seems true even of nonsense syllables where trials are the only measure of learning. However, it was noted after the experiment was under way that the time factor was present, even in the control condition with the use of the Hull drum, in that the subject had to anticipate the syllable in two seconds. Many subjects complained of this too-precise presentation which tended to "upset" them. Many claimed that they could say the syllables without the drum; could write them out, etc. Some went so far as to refuse to look at the drum but turned their heads and began reciting the syllables without the aid of it. They seriously objected to being forced to meet the condition.

4. None of the differences between mean *relearning* scores on different conditions was statistically significant. Seven out of eight measures in the case of the maze showed that relearning was deleteriously affected in the experimental conditions. The nonsense syllables showed somewhat better relearning scores under the experimental conditions, but the differences between the experimental and controlled conditions were not statistically significant; and in view of the objection given above little weight can be placed on these results.

5. With the exception of one case, the *sigmas* of the distribution

of scores showed a consistent increase under the experimental conditions.

6. The data were arranged to ascertain whether the amount of effect which was produced by the time factor would increase or decrease over the four-day period. The number of cases was necessarily small and the groups containing atypical individuals would influence the trends. However, it was thought that if the data showed any consistent trends, a basis might be established for formulating some generalizations. Examination showed that the only groups of measures which indicate a trend are the maze measures of Condition 1 and the measures of steadiness time. In these cases the tendency is for the scores to grow larger when the test is in any position except the first. In 13 out of 20 measures the last position has a higher cumulative score than the first. This might justify the generalization that the effect of the time limit and feeling of inferiority have a slight tendency to increase over the four periods.

7. Table 2, which gives a tabulation of statements gathered from the *introspective reports,* shows marked differences between the three conditions. Most of the statements were found in all three conditions, but their frequency tends to increase in the second and third conditions progressively. The increase did not show itself in statements of dislike for the tasks. More people under Condition 1 expressed dislike for the task than under the experimental conditions. In the experimental conditions their introspective reports were filled

TABLE 2

Frequency of Subjects' Statements Concerning Their Attitude

Number of persons expressing following attitudes

Conditions:	1	2	3
Temporarily inefficient	6	8	15
Distracting environment	10	14	19
Nature of tests		3	8
Fear			5
Dislike	8	1	3
Inferiority	6	4	16
Disgust		9	5
Distracting ideas		1	4
Other rationalizations			2
Absurdity of tests		6	3
Total	30	46	80

TABLE 3

BEHAVIOR NOTED DURING EXPERIMENTATION TABULATED IN ORDER OF FREQUENCY

1. Restlessness
2. Statements of inferiority feelings
3. Statements concerning time limit
4. Swearing
5. Laughing
6. Desire to quit
7. Tremor
8. Weakening of voice
9. Deep sigh
10. Statements of temporary inefficiency
11. Statements of hope of improvement

with statements offering excuses for their apparent lack of ability rather than expressing their boredom due to the tests. Statements of inferiority appeared most frequently, as one would expect, under Condition 3.

8. In Table 3 some of the behavior patterns exhibited are listed in order of their frequency of appearance. Such behavior appeared during both conditions but with greater frequency under the experimental conditions. Because of the necessity of taking records on the different tasks, the experimenter was unable to get an exact numerical estimate of the frequency of appearance of this behavior. From the results discussed above, the generalization can be drawn that marked differences appear in the subject's expressed attitude when a time factor alone is introduced, and that even greater differences appear when both a time limit and a suggestion of intellectual inferiority are present.

9. In order to make a more *analytical* study of those conditions in which a time limit and a suggestion of intellectual inferiority were introduced, Condition 2 was used as the control and Condition 3 as the experimental condition. Graphs were plotted of trials and errors per minute under the two conditions for the maze, steadiness test, and multiplication problems. These graphs (Figure 1) should show any influence which was exerted by the 6-minute time limit on any particular part of the learning or work period.

The average number of *trials* per minute in the maze was consistently higher in Condition 3 than in Condition 2. The curve for Condition 2 also is less irregular than that for the experimental conditions. The curve has the typical shape and is not presented here.

This allows the generalization that the subject requires more trials per minute throughout the entire maze-learning process when he is given a time limit and a suggestion that he is intellectually inferior.

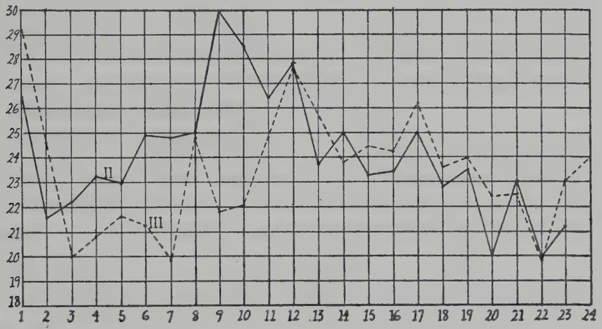
The *work curve* for multiplication (Figure 1) has a higher initial spurt under Condition 3, but falls rapidly and remains below that of Condition 2 for 14 minutes. Then the curves become nearly identical. The generalization that may be drawn from this is that, in multiplication, subjects working under a time limit and given a suggestion of inferiority start at a higher initial level, but their output rapidly falls below that of the control group; after the task is half completed, the groups tend to become identical in production per time unit. This relates the phenomena here discussed to the quantitative set demonstrated by Bills and Brown (1), the subjects seeming to prepare themselves for the increased demand under the experimental conditions.

The work curves plotted from the data of the steadiness test are rather typical, showing a warming-up and decrement. The two curves have the same shape, and show the same tendency found in learning curves, that of Condition 3 being consistently higher than Condition 2, and are thus not included here.

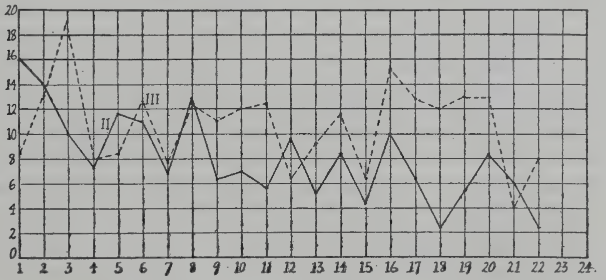
The *error curves* all are consistently higher under Condition 3. The error curve for the multiplication (Figure 1) under Condition 3 shows a lower initial point with a subsequent higher point than the curve of the control condition. In addition to this peculiarity the curve rises several times during the duration of the task; in fact, it shows no clear tendency in the direction of a decrement. It is worth noting here that the multiplication task turned out to be a practice situation rather than a work situation. The errors were greater in the beginning than later. The curve (Figure 1) showing errors per minute in the maze again brings out the general tendency of increased errors under the experimental condition. This leads to the generalization that the elimination of errors is retarded under a time limit and a suggestion of intellectual inferiority. For the two work tasks the errors show a general tendency to increase as the task progresses.

DISCUSSION

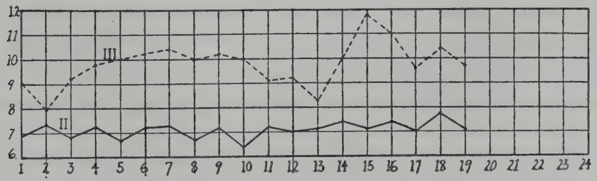
Judging from the behavior and general attitude of the subjects there is little doubt of the fact that a genuine emotion was induced in most of them under the experimental conditions. This emotional condition definitely affected the learning process as can be seen in the gross scores and the error curves. In the multiplication (Figure 1) the error curve for the control condition shows a steady drop, whereas the curve for the experimental condition does not show a tendency to drop. In fact, it hardly resembles a learning curve at all. It has an initial rise instead of an initial fall and rises again several times during the learning process. This seems to indicate that the initial fall in the curve may be partially due to an emotional condition which the individual overcomes.



Average number of multiplications per minute—Conditions II and III



Average error in multiplication per minute—Conditions II and III



Average error in maze per minute—Conditions II and III

FIGURE 1

In seeking an explanation for the effect which emotion has upon learning it might be well to remember that the physiological concomitants of emotion prepare the organism for overt behavior and not for ideational activity. The individual is energized and is impulsive; he finds it more difficult to represent his activity ideationally than to make the muscular response which vital processes prepare him to execute. In the light of this consideration, it is not surprising that we find so many more errors in the records of the subjects under the emotional condition. Even errors in the middle of the learning process might be expected. For example, in the maze, in addition to the correct responses which have been fixated, the subject often makes a number of extra random movements which are recorded as errors.

Some authors have considered the emotions important motives. Emotions may serve as great sources of motivation for action, but strong emotions can hardly be regarded as directors for problems which require deliberate activity. It is conceivable that as long as the emotion is just a mild but persistent stimulus it may act as a director or energizing force for ideational activity, but as soon as an emotion becomes so strong that impulsive behavior is unavoidable it retards rather than directs activity. The fact that "the after-effects of a connection" strengthen or weaken that connection or that an act is selected if it is successful has been well established and is known as the law of effect. However, it seems, in the light of the data here presented, that in addition to the after-effects of operation of a given specific connection, the effects or consequences of the whole learning situation are related to the fixation of correct responses and the elimination of errors. In this experiment many of the subjects maintained an unpleasant feeling as well as an attitude of inferiority and failure during almost the entire task. This was an effect of the set and the whole situation. The subjects realized that they were failing and this retarded learning. It therefore seems that in addition to the influence of the consequence of a specific connection on the fixation of that connection there is also the influence of the effect of the entire situation on the connections.[3] How this effect operates is a mattter of speculation at present. It is certainly true that if the subject maintains an attitude of failure or has an affective experience during the entire task, this is associated with each specific act in that it is a part of the sensory situation. How these associations retard learning is a matter for future investigation. It may be that the emotional condition and its concomitants fully account for the effect on the learning situation; or it may be that the emotional situation accounts for only a part of the retardation.

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[3] Dr. John A. McGeoch suggested that this may be considered another aspect of the law of effect.

Concerning the method in general, it seems that this experimental situation should prove a good one for the study of individual differences in personality and behavior in problem situations as well as a method for the study of emotional expression of individuals. By subjecting individuals to a standardized task as given here and recording their incidental behavior, it is possible that personality differences, overt and covert, in response to a problem situation would be evinced. It was noted that some subjects when asked to give an introspective report offered few or no rationalizations concerning their behavior during the tests, whereas others wrote pages. Some had relatively little to say during the tasks while others ceaselessly talked about the distracting environment or their temporary inefficiency. Still others laughed and belittled the tests. Quite a few swore, some apologizing, others not. The finding which seemed most interesting was that the subjects seemed to be off their guard concerning this incidental behavior because of the trouble they were having with the tasks and because of the pressure under which they were laboring.

Concerning emotional expression it might be noted that the experimenter has two means of measuring emotion by this technique: one by the number of errors which the subject makes in this task in excess of a control condition, and another by the number of movements and verbalizations which occur during the course of the task. As pointed out in describing the apparatus, a one-way vision screen was used in this experiment in order to note differences in movement frequencies under the two conditions. It was not possible to make records on both this and the tests, but a qualitative difference was noted under the experimental and control conditions. The work might be carried further to determine whether this method will serve as a means of measuring the behavior concomitants of a subjectively disturbed condition. Such a disturbance was reported by most subjects, and also expressed itself in their unsteadiness as shown by their scores.

This experimentation also raises a question concerning the validity of measuring intellectual abilities by the time-limit method, which is likely to introduce an emotional factor in some individual cases, invalidating the results.

The third condition of this experiment might be called an implicit "blame" condition which, in the light of past experiments, ought to produce better results than a control condition. Apparently the other complicating factors, such as a time limit and a rather strong emotional condition, tend to counteract any motivating effect of the conditions.

CONCLUSIONS

1. The introduction of a clock ringing periodically causes a consistent increase in errors in maze learning, multiplication, and motor coordination over the control condition, throughout the tasks. Time and trials are slightly affected in both directions, but more often in the direction of an increase.
2. The addition of a time limit and a suggestion of inferiority cause a still greater increase in these measures.
3. There is a slight indication that the effect of the experimental conditions becomes greater over the four days for some tasks.
4. There is a slight indication that retention is less under the experimental conditions.
5. The experimental conditions caused a measurable difference in the expressed attitude of the subjects, as well as a change in social behavior and an unsteadiness of movement.

SUMMARY

One hundred thirty-five subjects, divided into three groups, learned a maze and a list of 12 nonsense syllables, performed a motor task, and solved a group of multiplication problems. While one group went through these tasks an automatic clock buzzed each minute. In another group, in addition to the above set-up, the subjects were told that the average person could do the task in six minutes and at the end of that time an automatic interval timer rang. The third group worked under normal laboratory conditions. Errors in the task increased greatly with the introduction of the clock ringing each minute and increased more with the suggestion of intellectual inferiority. Time and trials were affected in both directions but tended to increase. There was a noticeable change in the expressed attitude of the subjects as well as a change in social behavior and steadi-

ness of movement due to the experimental conditions. If there was any change in the effect of the conditions over the four days it was in the direction of an increase. The experimental results evince that emotion affects learning and that a strong emotion cannot act as a motive in an intellectual task or a skill. There seems evidence for another aspect of the law of effect in terms of the effect of the consequences of the whole learning situation on the fixation of specific acts. The set-up suggests a method of measuring emotion and of studying personality under a problem situation. Timing the subject and blame do not always act as motivators.

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CERTAINS FACTEURS ÉMOTIFS DANS L'APPRENTISSAGE ET L' EFFICIENCE

(Résumé)

Cent trente-cinq sujets divisés en trois groupes ont appris un labyrinthe et une liste de 12 syllabes non-sens, ont fait une tâche motrice et ont résolu un groupe de problèmes de multiplication. Pendant qu'un groupe a accompli ces tâches, une horloge automatique a sonné chaque minute. Dans un autre groupe, outre l'arrangement décrit ci-dessus, on a dit aux sujets qu'une personne moyenne pourrait faire la tâche dans six minutes et à la fin de cette durée un appareil automatique d'enregistrement des intervalles a sonné. Le troisième groupe a travaillé dans des conditions normales du laboratoire. Les erreurs dans la tâche sont devenues beaucoup plus nombreuses avec l'introduction de l'horloge qui sonne chaque minute et sont devenues plus nombreuses avec la suggestion d'une infériorité in-

tellectuelle. Le temps et les épreuves ont été influencés dans les deux directions mais ont tendu à s'accroître. L'attitude exprimée des sujets a changé d'une manière marquée, ainsi que le comportement social et la fermeté du mouvement, ce qui a été dû aux conditions expérimentales. S'il y a eu un changement de l'effet des conditions dans les quatre jours, il a été dans la direction d'un accroissement. Les résultats expérimentaux montrent que l'émotion influe sur l'apprentissage et qu'une émotion forte ne peut agir comme mobile dans une tâche intellectuelle ou habileté. Il semble exister des témoignages d'un autre aspect de la loi de l'effet en termes de l'effet des conséquences de la situation entière sur la fixation des actes spécifiques. L'expérience suggère une méthode de mesurer l'émotion et d'étudier la personnalité dans une situation de problèmes. L'enregistrement du temps mis par le sujet et le blâme n'agissent pas toujours comme mobiles.

McKINNEY

GEWISSE AFFEKTIVE EINWIRKUNGEN AUF LERNEN UND LEISTUNGSFÄGIKEIT

(Referat)

Es wurden 135 Versuchspersonen in drei Gruppen gesondert. Alle 135 lernten ein Labyrinth durchforschen, lernten eine aus 12 sinnlosen Silben bestehenden Liste, führten eine motorische Aufgabe aus, und lösten eine Gruppe von Multiplizierungsaufgaben. Während eine der Gruppen diese Aufgaben ausführte, machte eine automatische Uhr jede Minute einmal einen summenden Laut. In einer anderen Gruppe bestanden die selben Verhältnisse, und es wurde ferner den Versuchspersonen gesagt, dass die durchschnittliche Vp. die Aufgabe in 6 Minuten erledigen konnte; am Ende dieser Zeit klang ein automatisches Zeitintervallenregistrierungsapparat [automatic interval timer]. Die dritte Gruppe arbeitete unter normalen Laboratoriumsverhältnissen. Die Fehlerzahl bei der Aufgabe nahm stark zu, wenn man die Uhr jede Minute schlagen lies, und noch stärker, wenn man die Idee der intellektuellen Unzulänglichkeit der Vp. suggerierte. Zeitverwand und Zahl der Versuche zeigten Abweichungen nach beiden Richtungen hin, neigten aber nach der Richtung der Vermehrung. Es zeigte sich eine merkbare Änderung in der geäusserten Einstellung der Vpp. und auch eine Änderung des sozialen Benehmens und in der Festigkeit der Bewegung unter Einwirkung der experimentellen Bedingungen. Wenn eine Änderung in der Einwirkung der experimentellen Verhältnisse im Laufe der vier Tagen überhaupt stattfand, so lag diese Änderung in der Richtung einer Zunahme. Die experimentellen Befunde weisen darauf hin, dass der Gemütszustand [emotion] das Lernen beeinflusst, und dass eine starke Gemütsbewegung bei einer intellektuellen Aufgab oder Tüchtigkeit [skill] nicht als Antrieb dienen kann. Es scheint Beweis vorzuliegen für das Bestehen einer anderen Seite des Einwirkungsgesetzes [law of effect] aus drückbar als Einwirkung der Folgen der Gesamtsituation auf die Fixierung spezifischer Tätigkeiten. Die experimentelle Einrichtung weist auf eine Methode zur Messung der Gemütsbewegung und zur Untersuchung der Persönlichkeit in einer Aufgabensituation hin. Die Bestimmung des Zeitverwandes der Vp. [timing] und Tadel wirken nicht immer anspornend.

McKINNEY

CHANGES ACCOMPANYING PRACTICE UPON SUCCESSIVE SAMPLES OF VERBAL MATERIAL*

From the Psychological Laboratories of the University of Missouri

JOHN A. MCGEOCH

PROBLEM

The problem of the changes which occur with the learning of successive samples of any given material has important implications in two directions. It bears directly upon the issues involved in transfer, and it pervades the interpretation of every experiment in which the same subjects are put through several conditions in a counterbalanced order and in which, therefore, each subject must learn several samples of the same material. Certain of the gross effects of such practice have been known since early in the history of experimental work upon learning, but there has been little analysis of the more specific effects or of their constancy in different groups of subjects.

This paper reports results upon four of the individual aspects of the major problem as follows: (1) the rate and amount of change with practice in the number of trials required to learn and in number of correct anticipations; (2) the influence of practice upon the relative variability of the number of trials taken to learn; (3) the relations between a criterion of one perfect trial and a criterion of three perfect trials; and (4) the constancy of these three factors in four samplings of subjects. The data have been taken mainly from an experiment on the relation between time-interval and degree of retroactive inhibition.[1] Related data from an experiment upon degree of interpolated learning and retroaction will be briefly examined under the caption Experiment II.

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[1] This experiment has been assisted by a Grant-in-Aid from the National Research Council.

EXPERIMENT I

Method. In the chief experiment from which the data are drawn, 80 college students, in four groups of 20 each, learned lists of 10 two-syllable adjectives each to a criterion of three successive perfect repetitions of the total list and relearned them after varying intervals. The words were exposed at a 2-sec. rate on an electrically driven drum and were learned by the anticipation method. An eleventh word was always added as the initial cue word, so that in each case 10 adjectives were learned. Record was taken of each anticipation or failure to anticipate.[2]

Each subject, upon beginning the experiment, learned two practice lists at a single sitting with an interval of 5 min. between the completion of the first and the beginning of the second. Thereafter the subjects were put through the five conditions of the experiment in a sequence which counterbalanced the order of the conditions and of the lists used on each condition. When the lists are arranged, not in this order but in the serial order of their occurrence through the experiment, half of the subjects take them in one order and the other half reverse the order of successive pairs. The practice lists were the same and in the same order for all subjects. These lists are homogeneous in content and very close together in difficulty. There is no reason to infer that the factor of the particular list has been influential in the conclusions drawn.

One group of 20 subjects (Group 4) learned under the *rest* condition in which a list was learned to the criterion at a single sitting and relearned after an interval of rest from any formal learning under laboratory conditions. The three other groups of 20 subjects each (Groups 1, 2, and 3) learned under the *work* condition in which they learned one list to the criterion and then went on to the learning of a second, interpolated, list for 10 repetitions. Group 3 learned its interpolated lists just prior to the relearning of the original lists, while the two other work groups learned theirs immediately following the learning of the original lists. They, likewise, relearned after an interval of freedom from laboratory acquisition. The intervals, which were the same for all four groups, were 20 min., 1 hr., 24, 48, and 144 hrs. In every case the learning for the next interval occurred on a day following the relearning of the preceding list, so that opportunities for positive and negative inter-serial effects at any given sitting are minimized. The total experiment, thus, lasted over a period of at least 14 days and it often lasted longer, since not all subjects were able to return on the day following each relearning for the learning of the next list. The intervals were a constant, but a slight margin was permitted between intervals.

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[2] Records were taken on special record blanks designed for use with the anticipation method. They were devised by Dr. Arthur W. Melton of Yale University and are printed by the Printing Office of the Yale Press.

In the work conditions each subject learned two practice lists and five experimental lists to a criterion of three successive perfect trials. He also learned five interpolated lists to 10 repetitions each. The rest condition was made comparable to the work condition by the insertion, after each relearning, of a balance list learned to 10 repetitions. This balance list could have no influence upon the relearning at any interval and it served to maintain equality of opportunity for practice between the rest and work conditions.

The four groups of this experiment have, therefore, had equivalent opportunities for practice at successive lists of adjectives. The practice at any single list has been massed, while from major list to major list it has been distributed. The data have been gathered by three different experimenters. One experimenter handled Groups 2 and 4, while Groups 1 and 3 each had a different experimenter. These experimenters had, in addition to training in experimental psychology, been given careful training by the writer in the use of the anticipation method, in the safeguards necessary in this kind of experimentation, and in the treatment of subjects. Their work was under careful scrutiny and the results have been obtained under the most constant conditions which we know how to impose upon experimenter, subject, and environment.

Rate and Amount of Change with Practice. It has been known since 1885 that the number of trials or the amount of time taken to learn decreases with practice at successive samples of the same material, but there has been little systematic study of this decrease. From the standpoint of experimental control, however, as well as from that of organized knowledge it is desirable to know as much as possible about these practice effects with a large number of different learning materials. The anticipation method offers excellent data for such a study on account of the relatively complete information which it provides regarding the status of the learning at each successive trial. In this paper, however, we are not concerned with the curves of memorization but with the total measures of learning. To this end we may employ (1) the number of trials required in order to reach the criterion and (2) the mean number of correct anticipations per trial. In all computations the criterial trials have been excluded.

In Table 1 the lists are arranged in serial order of practice without regard to the time interval after which they were relearned. The data of the table are, therefore, for the lists learned on the practice day and on the first to the fifth days of the main experiment. The letters A and B refer to the practice lists and the Roman numerals to the experimental lists. The results of the four groups have been presented separately in order to permit an examination of the extent of their uniformity. A question which always haunts the experimenter who employs small populations of subjects and relies upon statistical guarantees for the validation of his conclusions is whether such guarantees may honestly be accepted under the conditions which he may be using. Presentation of the data from four different groups, some of which have learned under as nearly identical conditions as can be obtained and all of whom have worked under similar conditions, while it will not crucially test

TABLE 1

Trials Required to Learn at Each Position of Practice

	Serial order							Percentage of gain
Group	A	B	I	II	III	IV	V	
To a criterion of one perfect recitation								
I M	25.3	17.2	13.9	11.5	10.8	10.9	9.0	64.4
σ	10.0	10.1	6.6	3.7	4.6	5.9	5.1	
II M	25.9	13.6	14.4	15.2	12.0	10.6	7.9	69.5
σ	11.2	6.8	7.1	7.2	5.6	4.8	3.4	
III M	26.0	15.2	14.4	12.8	11.8	7.4	7.6	70.7
σ	11.6	6.1	4.8	5.7	5.1	2.3	2.1	
IV M	23.9	13.3	14.2	11.3	12.6	10.4	9.1	61.9
σ	11.6	6.9	7.1	7.1	5.6	4.8	4.4	
To a criterion of three perfect recitations								
I M	37.5	23.9	19.1	17.9	16.8	15.9	11.6	69.0
σ	17.7	12.5	9.1	6.7	8.4	5.9	6.5	
II M	34.7	20.7	20.2	20.7	17.4	14.7	11.4	67.1
σ	14.9	9.9	8.5	10.8	8.2	7.2	5.7	
III M	36.9	24.3	20.7	19.1	17.5	10.8	10.4	71.8
σ	18.1	10.6	10.0	7.4	9.9	3.9	4.8	
IV M	32.3	21.7	20.0	17.2	18.2	14.8	12.3	61.9
σ	15.3	12.5	9.9	7.8	6.7	8.4	6.9	

the validity of statistical predictions from a small population, will provide an important view of the variability of the means obtained by this particular method from samplings of equal size.

The number of trials required to learn to both criteria drops sharply from the first to the second practice list and continues to fall slowly and with a high degree of uniformity from that point to the end of the experiment. The mean decrease in number of trials from Positions A to B is 41.5% for the lower criterion and 35.9% for the higher, while the mean percentage of decrease from Position A to Position V are 66.7 and 67.5 respectively. There is, thus, a negatively accelerated decrease in the number of trials taken to learn and this decrease amounts at the seventh position to two-thirds of the number required at the first position.[3]

The relatively great increase in speed of learning with a relatively small amount of practice reinforces what is already known regarding the incremental effects of practice upon successive samples of material and places a quantitative statement upon the amount of the increment with adjectives as material, permitting us to say that practice at complete learning and relearning of seven lists and the incomplete learning of five lists decreases by two-thirds the number of trials required for criterial learning. The sharpness of the deceleration of this practice effect is important for experimental procedure. Since a large percentage of the effects of practice occur during the learning of the first two lists, we may reasonably begin the experimental conditions after practice at two lists, expecting the remaining practice effects to be distributed satisfactorily by the counterbalanced order.

The uniformity of the results of the four groups is, considering the relatively small populations, rather great. The four have closely similar values at the first position in the serial order of practice, and, with no exception of importance, maintain this agreement throughout.

The mean number of correct anticipations per trial provides another measure of the influence of repeated learning. In terms of this measure, the data from which are shown in Table 2, all groups have lower values at the initial position than at any other.

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[3] The data for the interpolated and balance lists, of which all groups learned 5 to 10 repetitions each, are not shown. The trials are constant at 50 for each subject, and little would be added to the evidence by a presentation of the number of correct anticipations during these trials.

TABLE 2

MEAN CORRECT ANTICIPATIONS PER TRIAL TO A CRITERION OF THREE PERFECT

			Serial order				
Group	A	B	I	II	III	IV	V
I	5.35	5.50	5.37	5.84	5.86	6.15	5.80
II	5.38	6.33	5.88	6.06	6.37	6.33	5.93
III	5.31	6.50	6.24	6.52	6.78	6.23	6.36
IV	5.32	6.12	5.89	6.59	6.50	6.24	6.29
Mean	5.34	6.11	5.97	6.25	6.37	6.23	6.09

From the first to the second list there is a mean increase of .77 of an anticipation per trial, but increases thereafter are small and irregular. The highest point is reached at the fifth list learned. Thus, while the number of trials required to attain a criterion of three perfect repetitions decreases by two-thirds, the mean number of anticipations per trial increases. The greatest increase, that from the first practice to the third experimental position, is 19.2% of the value of the first. These two facts cooperate to give a forceful picture of a very considerable increase in speed of learning with relatively brief practice.

The mean numbers of anticipations per trial to a criterion of one perfect are somewhat less than those to a higher criterion, but the four groups agree as closely as they do in Table 2, and only the means for the 80 subjects need be reported. These means are 4.34, 4.83, 4.86, 5.14, 5.17, 5.22, and 5.36. They show an unbroken increase from position to position, although in the separate groups inversions occur. The percentage gain is 23.5.

Relative Variability. Pearson coefficients of variability have been computed for the numbers of trials taken to learn to both criteria. Inasmuch as these coefficients can be found readily, by anyone interested, from the data of Table 1, they will not be reproduced. These coefficients show no uniform tendency to increase or to decrease with continued learning. They fluctuate both from position to position and from group to group at a given position. The figures for the higher criterion are slightly larger than those for the lower, but the differences are scarcely large enough to be important. It is impossible to conclude that practice at successive lists has, as measured by this method, any progressive influence upon variability in trials required to learn.

This single method of examining the influence of practice upon individual differences is far from offering a complete analysis of the data with reference to this problem. A thorough analysis involves issues which do not fall legitimately within the scope of this paper. Since, however, the relative variability of the measures may be seen so readily from Table 1, this brief reference to it has been made.

Relation between the Two Criteria. The problem of what criterion to use in experiments upon the factors affecting learning faces every experimenter in the field. In experiments upon memorizing, either one perfect repetition or three perfect repetitions in succession has been the criterion usually employed and one often wishes to know to what extent the two yield similar results and whether the ratio between them is a constant or a variable. We have already seen that the effects of practice upon successive lists are closely similar in rate and percentage of gain for the two and that the relative variabilities are not discernibly different. We shall now inquire what is the ratio between them and what is the constancy of this ratio. The percentages obtained when the number of trials taken to learn to the lower criterion is divided by the number required to learn to the higher are given in Table 3. While these percentages vary somewhat from position to position, and from group to group, the variation is not great. The largest difference between the mean percentages of the four groups is that between positions 2 and 7 which is 8.1%. The percentages at the last position tend to be larger than any of the others, but in view of the absence of any hint of a regular increase one can hardly conclude that practice has had any effect upon the ratio between the two criteria. In general, slightly more than two-thirds as many

TABLE 3

Trials to a Criterion of One Perfect Divided by Trials to a Criterion of Three Perfect

	Serial order						
Group	A	B	I	II	III	IV	V
I	67.4	71.9	72.5	63.9	64.2	68.2	77.6
II	74.6	65.7	71.2	73.4	68.9	72.1	69.3
III	71.2	62.5	69.5	67.0	67.4	69.4	73.0
IV	73.9	61.2	71.0	65.6	69.2	70.2	73.9
Mean	71.8	65.3	71.0	67.5	67.4	70.0	73.4

TABLE 4
Mean Number of Correct Anticipations to One Perfect Trial Divided by the Mean Number to Three Perfect Trials

	Serial order						
Group	A	B	I	II	III	IV	V
I	51.4	57.7	49.6	38.3	39.0	41.0	41.9
II	63.1	52.9	58.5	64.2	54.7	63.9	64.0
III	58.5	49.4	56.2	51.6	57.4	57.2	63.1
IV	60.8	47.8	58.6	53.7	55.7	59.4	64.5
Mean	58.4	51.9	55.7	51.9	51.7	55.3	58.3

trials are required for learning to a criterion of one perfect as to a criterion of three perfect.

A second measure of the relationship between them is provided by the number of correct anticipations to each criterion. The relation involved can be seen most readily if we use the means of all correct anticipations instead of the mean number per trial, because here we wish to discover the relative degrees of total learning and not relative degrees of learning per trial. The percentages obtained by dividing the mean number of correct anticipations to the lower criterion by the mean number of the higher are shown in Table 4. The variation from group to group and from position to position is somewhat greater than in the case of trials taken to learn, but even so the uniformity is more striking than is the variability. The greatest difference between the means is 6.7%, which is small for data of this sort. The mean ratios are consistently smaller than are those for trials to learn, the average difference being about 15%. On the average, about 55% of the correct anticipations made in learning to a criterion of three perfect trials occur before the lower criterion of one perfect trial has been reached.

Experiment II

Whether the conclusions drawn in Experiment I are peculiar to lists of adjectives or whether they may be extended to other materials can be tested in one case with the data from an experiment (2) which employed lists of nonsense syllables learned by the same method as that used in Experiment I. Twelve college students, hitherto totally unfamiliar with the anticipation method and with experimental work on memorizing, learned to a criterion of three perfect trials in succession one list of 8 syllables from the Glaze (1) 0% group on each of three practice days prior to the

TABLE 5

MEAN VALUES FOR THE NONSENSE SYLLABLES OF EXPERIMENT II

Measure	Practice list I	II	Final list III	I
Tr. to 1 perf.	41.7±19.9	33.2±13.0	26.5±12.6	15.9±12.6
Tr. to 3 perf.	60.8±28.00	44.5±17.5	35.1±15.1	22.4±14.1
1÷3	68.5	74.6	75.5	70.9
V.-tr. to 1 perf.	47.7	39.1	47.9	79.2
V.-tr. to 3 perf.	46.0	39.3	43.0	62.9
Anticip. per tr. 1 perf.	3.49	3.41	3.86	4.09
Anticip. per tr. 3 perf.	4.46	4.22	4.56	4.96
1÷3 (total anticip.)	53.6	60.2	64.0	58.6

beginning of the main experiment. In the latter they learned, either partially or completely, under the conditions described elsewhere (2), 22 lists of syllables of mixed associative value. On the day following the completion of the main experiment they learned another list of 0% associative value. The order in which these 0% lists were learned was counterbalanced so that all lists had equal average position. Results comparable to those presented in Experiment I are summarized in Table 5.

From a comparison of these results with those obtained from Experiment I several conclusions emerge: (1) At the first practice position 71.9% more trials are required to bring the syllables to a criterion of three perfect than are taken to bring the adjectives to the same criterion. The syllables require 65.1% more trials than do the adjectives to attain the criterion of one perfect trial. (2) The percentage reduction shown by the syllables after practice at two lists of 0% syllables and 22 lists of mixed syllables is slightly less than that shown by the adjectives after practice at a total of 11 lists of adjectives. (3) The percentage relations, however, between the number of trials taken to learn the syllables to the two criteria are of the same order of magnitude as are those yielded by the adjectives and permit the generalization of the constancy of this relation to a second kind of learning material. (4) Relative variability fluctuates irregularly during the first three days of the practice but is markedly larger for the final list. Under these conditions and by this method individual differences are accentuated by practice. (5) As one would expect from the greater number of trials required to learn the syllables, the mean number of anticipations per trial is less at each criterion for the syllables than for the adjectives. Difference in length of list may also be a factor. There is a rather clear increase with practice in the case of the syllables. (6) The ratios between the total number of correct anticipations to the two criteria are of the same order of magnitude as are those yielded by the adjectives.

Summary and Interpretation

The changes which accompany the learning of successive lists of adjectives have been studied with four groups of 20 subjects each. All groups learned two lists during practice, and during the main experiment learned five lists to a criterion of three perfect trials and five other lists to 10 repetitions each. The major results are summarized in the following statements.

1. The number of trials required to learn decreases sharply from the first to the second list and more slowly thereafter. The total decrease amounts to two-thirds of the number required to learn the first list.
2. The mean anticipations per trial, to a criterion of three perfect, increases irregularly and by a much smaller amount than the trials decrease. The greatest increase is 19.2%. The means to a criterion of one perfect trial show a more regular increase which amounts to 23.5%.
3. Relative variability, as measured by the Pearson coefficient of variability, fluctuates somewhat but shows no regular alterations with continued practice.
4. In terms of trials, there is a roughly constant relation between the criteria of one perfect trial and three perfect trials. Slightly more than two-thirds as many trials are required for learning to the lower than to the higher criterion and this relation is unaffected by practice.
5. The same relative constancy of relation appears when the total number of correct anticipations is employed. About 55% of the correct anticipations made in learning to the higher criterion occur before the lower criterion has been reached.
6. The four groups agree with a uniformity which is remarkable when one considers their relative smallness and the multiplicity of factors which are operating. This agreement is, within the limits of these experiments, an effective answer to those who question the generality of conclusions from the samples used in most experiments in the psychology of learning.

The results of a related experiment in which lists of nonsense syllables of 0% associative value were the materials used have been placed in comparison with those obtained with adjectives. Three lists were learned at the rate of one per day during the preliminary practice. There followed the learning, either partially or completely, of 22 lists of mixed associative value, after which another list of 0% syllables was learned. A summary of these results and a comparison between those of the syllables and of the adjectives follow:

1. The number of trials required to learn decreases sharply from the first to the second list and then more slowly. The percentage of decrease from first practice list to final list is slightly less than that found with the adjectives, in spite of the fact that the amount of practice as measured in trials taken is considerably more than twice as great.
2. The relative variability of the syllable lists is, with the exception of the large value for the final list, of the same general size as of the adjective lists.
3. The mean number of correct anticipations per trial is smaller than with the adjectives and there is a definite tendency for the means to increase with continued learning.
4. The ratios between the numbers of trials taken to learn the syllables to the two criteria are of the same order of magnitude as are those for the adjectives and indicate that this is a generalized relationship for these two materials. The corresponding ratios between the numbers of correct anticipations are roughly of the same magnitude as are those of the syllables, and in both cases they are relatively unaffected by practice.

These results upon the influence of the learning of successive lists demonstrate that amount of transfer is a function of the material learned, even when all lists within a given experiment are composed of similar materials, although under the conditions employed, it uniformly appears at a negatively accelerated rate. The question of the factors upon which this difference depends is basic to an interpretation of transfer phenomena and one which sorely needs investigation.

The relationships between the two criteria are of importance with respect to two different considerations: (1) Their constancy at different points in the learning of successive lists instances a uniformity which is one effective answer to critics who doubt the generality of the results obtained from small samplings of subjects for other comparable groups. (2) Methodologically, this constancy of relation indicates that, for many problems where interpretation is to be made upon the basis of a comparison of mean scores, either criterion may be used. Whether degree of learning is measured in terms of number of trials or of anticipations, however, the lower criterion yields a much smaller degree of learning. This is especially true of the latter measure. The rather great difference between the two criteria is a matter of importance in determining which criterion may be used most satisfactorily in the solution of a given experimental problem.

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LES CHANGEMENTS QUI ACCOMPAGNENT L'EXERCICE AVEC DES CHOIX SUCCESSIFS DE MATÉRIEL VERBAL

(Résumé)

Quatre groupes, chacun de 80 sujets, ont appris selon la méthode d'anticipation 7 listes de 10 adjectifs jusqu'à un critérium de 3 épreuves parfaites de suite. On a appris cinq autres listes jusqu'à 10 répétitions chacune, mais on ne considère que les résultats des 7 listes principales. Le nombre d'épreuves nécessaires pour l'apprentissage est beaucoup plus petit pour la deuxième liste que pour la première, mais diminue plus lentement après. La diminution totale est les deux tiers du nombre nécessaire pour l'apprentissage de la première liste. Les anticipations moyennes pour chaque épreuve s'accroissent, mais de plus petites quantités. La variabilité relative des épreuves ne montre aucune altération régulière avec l'exercice continu.

Il y a une relation assez constante entre le nombre d'épreuves nécessaires pour atteindre les critériums d'une épreuve parfaite et de trois épreuves parfaites. Il faut un peu plus que les deux tiers du nombre d'épreuves pour apprendre jusqu'au critérium inférieur qu'il le faut pour apprendre jusqu'au critérium supérieur et cette relation n'est pas influencée par l'exercice. Environ 55% des anticipations correctes faites dans l'apprentissage au critérium supérieur ont lieu avant que l'on arrive au critérium inférieur. Les quatre groupes s'accordent avec une uniformité qui est une réponse importante à faire à ceux qui doutent de la généralité des conclusions tirées des petits choix employés dans les expériences dans la psychologie de l'apprentissage.

Ces résultats sont en général confirmés par ceux d'une expérience qui a employé des listes de 8 syllabes non-sens.

McGeoch

ÄNDERUNGEN, DIE MIT DER ÜBUNG AN AUFEINANDERFOLGENDEN PROBEN MIT VERBALEM LERNSTOFF EINHERGEHEN

(Referat)

Vier Gruppen, bestehend aus je 80 Versuchspersonen, lernten mit der Voraussagungsmethode [anticipation method] 7 Listen von je 10 Eigenschaftswörtern auswendig,—immer bis die Liste drei Mal nacheinander richtig wiederholt wurde. Es wurden 5 weitere Listen auswendig gelernt bis sie 10 Mal nacheinander richtig wiederholt werden konnten, aber es werden hier nur die Befunde mit den 7 Hauptlisten berücksichtigt. Die Zahl der zum auswendig lernen nötigen Versuche nahm von der ersten zu der zweiten Liste scharf ab, und danach langsamer. Die Gesamtabnahme beträgt Zwei-drittel der zum auswendiglernen der ersten Liste nötigen Wiederholungen. Die Durchschnittszahl der Voraussagungen in jedem Versuch nimmt zu, aber in geringerem Grade. Die relative Veränderlichkeit [variability] der Versuche erweist mit fortsetzender Übung keine regelmässige Änderung.

Es besteht eine im Überschlag konstante Beziehung zwischen der zur Erzielung von *einer* richtigen Wiederholung und von *drei* richtigen Wiederholungen nötigen Versuchszahl. Zur Erzielung der niedrigeren Norm [Standard] sind etwas mehr als Zwei-drittel so viele Versuche nötig, wie zur Erzielung der höheren Norm, und diese Beziehung wird durch die Übung nicht beeinflusst. Ungefähr 55% der richtigen Voraussagungen finden vor der Erreichung der niedrigeren Norm statt. Die vier Gruppen stimmen mit einander mit einer Gleichförmigkeit überein, die denen, die die allgemeine Gültigkeit der aus den, in Versuchen in der Psychologe des Lernens verwendeten, kleinen Probegruppen gezogenen Schlüsse in Frag stellen, eine wirksame Antwort darbietet.

Diese Befunde werden im Allgemeinen von den Befunden aus einem Experiment, in dem Listen von 8 sinnlosen Silben verwendet wurden, bestätigt.

McGeoch

AFFECTIVITY IN RELATION TO BREATHING AND GROSS BODILY MOVEMENT*

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In the present experiment a carefully selected set of pictures was used to call out different kinds of feelings and emotions. A study was made of the time which elapsed before the emotion began, the duration of the emotion, and the amount of breathing and gross bodily movement which occurred while the different feelings and emotions were present. The principal purpose of the experiment was to determine the relation between, first, the feelings and emotions which were called out by the visual stimuli, and, second, the amount of breathing and gross bodily movement. We do not regard the affectivities as the causes of physiological processes or the physiological processes as the causes of affectivities, and we have not studied the "bodily expressions" of mental activities any more than we have studied the "mental expressions" of bodily activities. We have been primarily concerned with the experimental results themselves rather than with any one of the numerous theories of feelings and emotions.[1]

Material

An attempt was made to secure pictures that would call out the emotions most commonly experienced in everyday life. Several hundred magazines and books were also examined with the purpose of obtaining pictures that would produce definitely unpleasant and definitely pleasant emotions. It was assumed that the same picture would frequently call out different emotions in different people, and we tried to select pictures which dealt with definitely emotional subjects. We tried out about 100 pictures in the preliminary experiment with eight S's and in the main experiment which is described in the present paper. We did not continue to use a

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[1] References to some of the related investigations may be found in Kellogg (3) and Jacobson (2).

picture if it failed to produce an emotional response fairly regularly. The most satisfactory visual stimuli were pictures of war horrors from a book on peace propaganda, amusing and humorous pictures from the magazine, *Ballyhoo,* pictures of beautiful women and handsome men from moving-picture magazines, some clinical varieties of mental deficiency, pictures of a few types of girls in scant and suggestive clothing, pictures of attractive babies, religious pictures, and attractive outdoor scenes. Each of the different kinds of pictures was scattered throughout the set, and the same kind of picture did not occur twice in succession for any S during the experiment. Each picture was mounted on a 10 x 10″ card, and in the set of cards each pair of adjacent pictures was separated by a blank card.

Apparatus

The apparatus used to measure gross bodily movement was the same as that which we had devised and used in a previous study (1). During the experiment S was seated in a specially constructed chair which was mounted on springs, and the chair was connected with recording apparatus in such a way that a quantitative measure could be obtained of his gross bodily movements during a given interval of time. A number of electric contacts was made whenever S made any definite movement of his body, and these contacts operated two electric counters. The movement record was a measure of S's fidgetiness, nervous restlessness, physical composure, or persistence of postural attitudes.

A part of the apparatus which was used to measure breathing was an adjustable leather belt 2¼″ wide which fitted closely around the S's body in the general region of the epigastrium. The belt was kept moderately tight and pressing against the body by a stout rubber band inserted in the left front section of the belt. The belt was prevented from slipping down by four cloth strips which were evenly spaced around the S and were attached to the belt at their lower ends and pinned to the S's clothes at their upper ends. The contact apparatus was inserted in the belt and was the front section of it. This apparatus made a series of electric contacts whenever the total length of the belt changed, and the number of contacts made was proportional to the change in circumference of the body. The electric contacts operated an electric counter, and the amount of breathing during a given interval of time could be registered automatically.

The construction of the contact apparatus was as follows. A rigid curved aluminum frame 6″ long and 2¼″ wide was attached to, and was continuous with, the end of the belt at the right front, and it was attached to, and was continuous with, the end of the rubber band at the left front. The other (left) end of the rubber band was attached to the end of the belt at the left front. Beginning in front and going around the S in the clockwise direction, the three different sections of the apparatus were, first, the curved aluminum frame the back of which was against the S's body, second, the adjustable leather belt which extended from the right front around the body to the left front, and, third, the rubber band which extended from the end of the belt at the left front to the left end of the curved aluminum frame in front. The length of the rubber band would change with any change in the distance around the S's body.

The contact mechanism was mounted on the front side of the aluminum frame, and it was protected from the S's clothes by guards extending forward horizontally from the upper and lower edges of the frame. A vertically placed pinion shaft 3/16″ in diameter was mounted on the frame, and it carried a small gear at the lower end and a much larger contact sprocket-wheel at the upper end. The small gear meshed with a larger gear that was driven, and the driven gear was connected on a small shaft with a coil (clock) spring of adjustable tension. The contact sprocket-wheel on the upper end of the pinion shaft was 1½″ in diameter and had 28 teeth which made contact with a V-shaped deformation near the left end of a thin copper contact brush 2¼″ long which was mounted on an insulated contact standard at the right.

The central portion of the pinion shaft was bare for a distance of 13/16″, and the end of a strong ribbon, ¾″ wide, was sewed and securely attached to the central portion of this shaft partly with the aid of several small holes in the shaft. The ribbon was wound several times around the shaft against the tension of the coil spring, and when the belt was adjusted and in place the tension of the spring held the ribbon taut. The free end of the ribbon, like the left end of the stout rubber band, was attached to the left end of the belt by a thumb-screw clamp. When the distance around the S's body increased, the rubber band became longer and the ribbon was unwound from the pinion shaft and rotated it against the tension of the coil spring. The sprocket-wheel rotated with the pinion shaft and made a series of electric contacts with the brush, and these contacts operated the electric counter.

The contact apparatus when properly adjusted was sensitive enough to record some breathing movements practically all of the time. The breathing record that was secured was an approximate measure of the total amount of S's breathing, but no measure of inspiration as compared with expiration was obtained for any period of time. The apparatus was not uncomfortable to wear. Practically all S's quickly became accustomed to it, and they would not pay any special attention to it or to their breathing movements. This apparatus has certain advantages over the various other devices that have been used, the principal one being that a score for breathing could be readily obtained without any trouble or delay.

A board was attached to the arms of S's chair, and a pile of about 30 picture and blank cards rested on this board. A telegraph key which S used to signal E was fastened to the board near its far left corner.

The experiment was carried out in a room 18 feet long, and E and S were at the opposite ends of this room. The recording apparatus was screened from S, and he could not see any of the wires that connected the chair apparatus and the belt mechanism with the three counters. The counters were covered on all sides with several layers of sound-absorbing material, and a slow-speed motor was always running while the counters were recording the breathing and gross bodily movements. The sound-absorbing material and the sound of the motor made it impossible for S to hear the counters, and in most cases he did not know that quantitative measures of his breathing and gross bodily movements were being made.

PROCEDURE

The first-named author was the experimenter for all S's, and the procedure outlined below was strictly followed.

1. S was seated in the chair apparatus, the breathing apparatus was attached and adjusted, and the board on which the large cards were to be placed was fastened to the arms of the chair.

2. E explained that the experiment was principally concerned with weak emotions and only with those which S actually had while he was looking at the pictures. S was asked to distinguish carefully between the emotions that someone might think should be evoked

by the pictures and the emotions that he actually had during the experiment.

3. S was given a list of emotions and a scale for grading feelings. He was practiced in the use of the signal key, and the sound of the motor was demonstrated to him.

4. He was given a fairly complete set of typewritten instructions, and he studied these directions carefully for 10 or 15 minutes. He was then questioned on the procedure, but considerable further explanation was necessary before the experiment could begin.

5. The detailed procedure which was followed in one trial with a single picture was as follows:

a. E said "Ready."

b. S turned the blank card face down to the right with the right hand.

c. S promptly read aloud the number of the picture in the upper left corner of the card, and E started a stop-watch when this number was read.

d. After reading the number aloud, S read silently any writing on the exposed picture, and he continued to look at it, to concentrate his attention on it, and to permit any thoughts and affective experiences to come to mind that were natural to him. The somewhat passive attitude that he was asked to adopt was similar to that of a person looking at a moving-picture performance. One who has this attitude does not attempt to call up any particular emotions, he does not try, for example, to be amused or depressed, but several different affective experiences are ordinarily called out principally as a result of the visual and auditory stimulation. The point was emphasized that we were concerned with the emotions that S actually had in the experimental situation, and not with the individual causes of the emotions or with any past experiences that S may have had which caused his particular affective responses. The principal assumption in the present experiment was that the S's introspection, or his restrospective report, is the most accurate and reliable indication of the presence of any affective activity.

e. S pressed the telegraph key with his left hand when any emotion began, and he held the key down as long as the emotion continued. A light remained on in front of E while S held down the key. S released the key promptly when the emotion disappeared. If the emotion returned, or if some other emotion appeared, S pressed the key again and held it down as long as any emotion was present. The stop-watch was running continuously in front of E, and E noted the time when the emotion appeared the first time, the time when it disappeared, and the total time that any later emotions were present.

f. E started the motor when S pressed the key the first time, and the motor was allowed to run for 20 sec., this interval of time being measured by the stop-watch. The three counters registered S's breathing and gross bodily movements only during this interval of 20 sec. The purpose of the motor was to help keep S from hearing the counters and to prevent him from knowing that quantitative measures of his bodily reactions were being made.

g. When S heard the motor stop he promptly turned the picture card over face down to the right with the right hand. If the emotion ceased as soon as the picture was out of sight, S released the key with his left hand; but the key was held down as long as the emotion continued, regardless of whether the picture was visible or not.

h. When the motor had stopped running, when the picture had been turned over, and when the key had been released the last time, S took his time in giving a careful oral retrospective report of the name or names of the emotions that were present while the motor was running. S was asked not to try to think of the names of the emotions before this time. He deliberately and carefully named the emotion that had been present, and the following list of suggested emotions, arranged as shown, was always before him for his convenience.

A LIST OF EMOTIONS

AVERSION
DISLIKE

DISGUST
REPULSION

PITY
SYMPATHY

DESPAIR
HOPELESSNESS

SADNESS
SORROW

ANGER
ANNOYANCE

CURIOSITY
FASCINATION
INTEREST
WONDER

AFFECTION
AMOROUSNESS
LOVE
PASSION
TENDERNESS

ADMIRATION
AESTHETIC PLEASURE
DELIGHT
PLEASURE

AMUSEMENT
HUMOROUSNESS

FEAR
HORROR

HATE

HUNGER
THIRST

We began the experiment with an arbitrary list of emotions, and changed the list several times so that it would include only those emotions that were most frequently used by the S's. The above list is the last revision that was made. S was asked to regard the list only as suggestive and to use any other names if they seemed to be more accurate designations of his emotions. If S had experienced two or more emotions he named one emotion as the most predominating or prominent and a second emotion as second in importance. E recorded the names of only two emotions, and only the most prominent one was used in the treatment of the data.

E had the S's list of suggested emotions before him, and also an additional list of emotions which was composed of the following names: adoration, amazement, anxiety, awe, bashfulness, boredom, contentment, dejection, desire, elation, embarrassment, ennui, envy, excitement, fatigue, grief, happiness, impressiveness, joy, longing, pain, peacefulness, pride, resentment, reverence, scorn, serenity, shame, terror, and worry. On several occasions near the beginning of the experiment when S gave the names of some emotions but thought the names were not entirely satisfactory, E offered to help by suggesting several of the names in the above list that were similar in meaning to those which S had given. On each of these occasions E would ask S to hold up the picture so that he could see which picture had just been used. By this indirect method all of the S's were led to believe that E did not know which picture was being used at a given time, and in this way the S's frankness in naming his emotions was greatly increased. S did not know that E had a typewritten list of the numbers and titles of the pictures before him, and the S's commonly said they thought that E could not know which picture was being used because one could not remember the numbers of all of the pictures. By following the reactions of the S's to the various pictures during the experiment we were able to make a number of at least interesting observations on the S's feelings and emotions in relation to their general personality characteristics.

i. S gave a grade for the feelings that had been present while the motor was running, using one of the numerical grades in the following scale:

SCALE FOR GRADING FEELINGS

Quite pleasant	+3
Moderately pleasant	+2
Slightly pleasant	+1
Indifferent	0
Slightly unpleasant	—1
Moderately unpleasant	—2
Quite unpleasant	—3

j. E made a record of the trial, and this record included the number of the picture, the number of seconds which elapsed before the emotion began, the duration of the emotion, the name of the emotion, the grade for feelings, the score for the amount of breathing during the interval of 20 sec., and the score for the amount of gross bodily movement during the same period of time. S rested and waited for the next "Ready" signal while these data were being recorded.

6. If no emotion of any kind appeared, S did not press the signal key at all; and, after waiting 20 sec. from the time S had spoken the number of the picture, E started the motor and at the same time started the apparatus for recording breathing and gross bodily movements. The motor ran and the counters recorded the number of breathing and movement contacts for a period of 20 sec. When S heard the motor stop he turned the picture card over and gave a grade for the feelings which had been present while the motor was running.

The above procedure was quite difficult for the S to follow at first, and it required such close attention at all times that they were not able to spend much total time in thinking about their breathing and gross bodily movements. These bodily reactions seemed for the most part to be involuntary.

At the end of the experiment each S was asked a number of questions to determine his insight into the experiment. The large majority of them did not know that quantitative measures were being obtained of their breathing and gross bodily movements. Most of them did not know the reason for using the belt apparatus, and several thought that it was to record reactions of the heart. All of the S's said that they had been conscious of their breathing and gross bodily movements only a small percentage of the time.

The experimental period lasted two hours, and fairly complete records were obtained for about 70 different pictures in the case of 25 out of 32 S's. The pictures were rotated by having each S begin in the series where the previous S had left off. The record of a single trial was omitted in a few cases when the instructions in regard to pressing the key and turning the picture over had not been properly carried out.

SUBJECTS

The 32 S's were undergraduate and graduate students, and their age and sex distribution was as follows:

Age	Men	Women
37	1	
—		
30		1
—		
27		1
26	1	
25	1	
24	1	1
23		2
22	4	1
21	2	2
20	2	3
19	3	2
18	1	2
17	1	
Total	17	15

RESULTS

The data which were recorded for one S's reactions to a single picture in a single trial were (1) the number of the picture, (2) the number of seconds which elapsed from the time S saw the picture until the emotion began, (3) the duration of the emotion in seconds, (4) the name of the emotion, (5) the grade for feelings, (6) the breathing score, and (7) the movement score. The absolute sizes of the latent time, duration, breathing, and gross bodily movement scores depended upon a number of factors such as the mental set of the S, his mood, sex, general physical make-up, and also the particular interpretation which he placed on some of the instructions. Since factors of this kind caused a certain amount of variation among different S's, we have not used the absolute sizes of the scores for items (1), (2), (6), and (7), but have transmuted these numbers into percentage scores on the basis of each S's results. In the latent time scores in item (1), for example, we have given each of the highest 10% of the time scores for an individual S a transmuted score of 10, each of the second 10% of the time scores for an individual S a transmuted score of 9, and so on until each of the lowest 10% of the time scores for an individual S was given a transmuted score of 1. If latent time measures were obtained with 60 pictures for an individual S, each of the 6 highest time scores was changed to 10, each of the next 6 time scores was changed to 9, and so on until each of the 6 lowest time scores for this S was changed to 1. If, for example, the transmuted time scores of a certain picture on a given trial is 10, then on this trial the time score is in the highest 10% of all of the time scores obtained with this S. All of the duration, breathing, and gross bodily movement scores have been transmuted in the same way into scores of 10, 9, 8, 7, 6, 5, 4, 3, 2, and 1 on the basis of their 10-percentile ranks among all of the duration, breathing, and gross bodily movement scores respectively for each S.[2]

The principal results of the experiment have been brought together in Tables 1, 2, 3, 4, and 5. It should be remembered throughout that the results in these tables on latent time, duration, breathing, and gross bodily movement are given in terms of the 10-percentile transmuted scores for each S.

Table 1 shows the relation between the transmuted scores for latent time and the transmuted scores for duration of the emotions. There were, for example, two trials in which the latent time score was 1 and the duration score was 1, five cases where the latent time score was 1 and the duration score was 2, etc. The numbers of cases and the averages are given in the two columns on the right and in the two columns at the bottom. There is a definite negative correlation between the scores for latent time and for duration. As the latent time scores increase, or as the speed of the emotions decreases, the average duration scores decrease. There is a clear tendency for very long latent time scores to occur with very short duration scores.

Table 2 gives the relation between the 10-percentile scores for

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[2] For considerable assistance in the calculation of these results the writer is under obligation to Robert Reichenbaum and Emily Perlman.

TABLE 1

THE RELATION BETWEEN THE SCORES FOR LATENT TIME AND DURATION OF THE EMOTIONS

		Duration											
		1	2	3	4	5	6	7	8	9	10	N	Av.
Latent time	1	2	5	13	8	11	15	20	23	23	38	158	7.21
	2	13	11	13	19	20	54	16	43	32	36	257	6.53
	3	12	12	17	23	22	22	35	16	19	23	201	5.95
	4	4	14	16	15	15	16	16	26	19	12	153	6.03
	5	16	23	24	31	29	36	23	38	28	16	264	5.67
	6	10	11	21	16	11	23	14	11	11	12	140	5.41
	7	17	20	18	25	20	32	13	20	12	21	198	5.41
	8	23	22	29	21	22	25	16	20	13	14	205	5.01
	9	37	38	22	15	17	17	15	11	6	7	185	4.02
	10	65	37	19	22	11	17	6	10	5	4	196	3.31
	N	199	193	192	195	178	257	174	218	168	183	1957	
	Av.	7.57	6.88	5.97	5.76	5.43	5.13	4.79	4.66	4.27	4.05		

TABLE 2

THE RELATION BETWEEN THE SCORES FOR BREATHING AND GROSS BODILY MOVEMENT

		Gross bodily movement											
		1	2	3	4	5	6	7	8	9	10	N	Av.
Breathing	1	24	19	24	7	29	17	17	23	15	4	179	4.96
	2	26	29	28	22	37	16	17	15	16	17	223	4.89
	3	24	17	41	12	19	27	20	21	19	16	216	5.21
	4	16	22	30	9	24	12	13	15	14	15	170	5.09
	5	18	32	24	26	37	25	19	30	24	24	259	5.49
	6	15	19	27	9	27	18	30	13	13	13	184	5.29
	7	13	22	25	12	15	17	16	15	16	23	174	5.53
	8	13	19	21	8	27	15	15	28	18	20	184	5.77
	9	14	20	34	12	25	17	21	23	29	24	219	5.77
	10	10	14	13	9	29	15	20	27	22	29	188	6.34
	N	173	213	267	126	269	179	188	210	186	185	1996	
	Av.	4.70	5.24	5.18	5.18	5.35	5.32	5.63	5.76	5.89	6.33		

TABLE 3

THE LATENT TIME, DURATION, BREATHING, AND GROSS BODILY MOVEMENT SCORES FOR DIFFERENT GRADES OF FEELING

Grades of feeling	Latent time		Duration		Breathing		Movement	
	N	Av.	N	Av.	N	Av.	N	Av.
+3	226	4.23	226	6.98	228	5.64	227	5.40
+2	384	5.07	384	5.77	379	5.55	384	5.59
+1	390	5.93	390	4.41	391	5.33	407	5.56
0	212	6.79	212	3.63	306	5.35	313	5.33
—1	268	6.54	268	4.50	255	5.54	271	5.42
—2	226	5.25	226	6.16	224	5.26	228	5.16
—3	243	4.17	243	7.41	233	5.66	238	5.47

breathing and the 10-percentile scores for gross bodily movement. There is a definite positive correlation between the breathing and gross bodily movement scores, but there are several irregularities in the size of the average scores.

Table 3 gives the average latent time, duration, breathing, and gross bodily movement scores for the different grades of feeling from quite pleasant, +3, to quite unpleasant, —3. These calculations have been based on all trials irrespective of whether the picture called out an emotion or not. The number of cases is shown on the left of each average score in the table. The average latent time score of 4.23, for all trials where the grade for feelings was +3, is based on 226 cases, and the average latent time score of 6.79, for all trials where the grade for feelings was 0, is based on 212 cases. There is no reliable difference between the average latent time scores for pleasant and for unpleasant feelings, but there is a marked and regular tendency for the latent time to increase and the speed of the emotions to decrease as the numerical values of the feeling grades decrease. The middle column shows that unpleasant emotions have a longer duration than pleasant emotions. As the strength of the feelings increases, either in the pleasant or in the unpleasant direction, the average scores for the duration of the emotions show a marked and regular increase. The results on breathing show that approximately the same amount of breathing occurs for the different grades of feeling, all of the average breathing scores ranging between 5.26 and 5.66. The same may also be said of the movement scores. There is no reliable difference between the average movement scores for pleasant and for unpleasant feelings, or for weak and for strong feelings. By means of the data given in Table 3 a comparison can be made between, first, the correlation between feelings and latent time and duration scores, and, second, the correlation between feelings and breathing and movement scores. There is a definite positive relation between feelings, on the one hand, and the latent

TABLE 4

THE LATENT TIME, DURATION, BREATHING, AND GROSS BODILY MOVEMENT SCORES FOR DIFFERENT EMOTIONS

	Latent time		Duration		Breathing		Movement	
Emotion	N	Av.	N	Av.	N	Av.	N	Av.
Horror	83	**4.31**	83	**7.27**	80	5.93	83	5.13
Fear	19	*4.53*	19	5.58	17	5.76	19	*4.60*
Disgust	66	*4.61*	66	*6.09*	61	5.41	65	5.51
Repulsion	101	**4.43**	101	**7.10**	98	5.38	98	5.42
Aversion	42	*6.07*	42	5.00	41	5.22	42	5.00
Dislike	83	**6.54**	83	**4.16**	81	5.48	83	5.60
Annoyance	55	*6.13*	55	**4.33**	49	5.02	55	5.47
Anger	27	**4.26**	27	**7.46**	27	*6.36*	26	5.31
Sadness	24	5.46	24	**6.67**	23	*4.96*	24	5.17
Sorrow	23	5.57	23	**7.04**	22	5.32	22	5.86
Hopelessness	16	*6.44*	16	5.50	16	5.88	15	**4.40**
Despair	16	5.00	16	*6.31*	16	*4.69*	16	5.31
Pity	78	5.72	78	5.96	76	5.66	77	*4.79*
Sympathy	37	5.90	37	5.38	36	5.67	36	5.72
Hunger	33	**4.24**	33	*6.21*	33	*6.03*	33	*6.39*
Interest	187	*6.49*	187	*4.55*	181	5.14	184	5.49
Curiosity	72	**6.96**	72	5.11	69	*4.99*	71	*6.04*
Pleasure	135	5.63	135	*4.63*	131	5.04	134	5.43
Aesthetic pleasure	54	5.41	54	5.20	49	*4.94*	54	5.20
Delight	55	5.13	55	5.82	52	5.35	55	*6.00*
Fascination	64	5.19	64	5.50	62	*4.74*	64	5.02
Admiration	129	5.69	129	5.46	122	5.40	129	5.18
Amusement	164	5.23	164	**4.29**	159	5.79	159	*6.01*
Humorousness	60	**4.45**	60	5.42	60	*6.17*	60	5.90
Tenderness	62	5.05	62	5.73	59	5.69	61	5.41
Affection	53	5.02	53	5.70	51	*4.78*	53	*4.81*
Love	30	**3.60**	30	**7.00**	30	**6.57**	30	5.87
Amorousness	21	**3.95**	21	**6.52**	21	*6.33*	21	5.57
Passion	37	*4.73*	37	**7.17**	37	*6.49*	36	5.61

time and duration scores, on the other; but a large or a small amount of breathing or movement may be present with quite pleasant, indifferent, or quite unpleasant feelings.

Table 4 gives the average latent time, duration, breathing, and gross bodily movement scores for the 29 most frequently reported emotions. The emotions are grouped and the groups are arranged in order from fairly unpleasant to fairly pleasant. A much better idea may be obtained of the reliability of the different scores when the scores for similar emotions are placed side by side. The number of cases is the same in all cases for latent time and duration, but measures were occasionally lacking for breathing and movement on account of the apparatus being temporarily out of order. The total number of cases is small for fear, hopelessness, and despair. The average of all of the central tendencies given in the table is approximately 5.50.

If we consider all of the scores below 5.00 and above 5.99 as significant, and use italics when the scores are below 4.50 and above 6.49, the general nature of these results may be more clearly appreciated in the following summary which is based on all of the average scores except those falling between 5.00 and 5.99 inclusive. The longest latent times were obtained with dislike and curiosity, and the shortest latent times with horror, repulsion, anger, hunger, humorousness, love, and amorousness. The emotions which lasted the longest were horror, repulsion, anger, sadness, sorrow, love, amorousness, and passion, and those which lasted the shortest time were dislike, annoyance, and amusement. The largest amount of breathing occurred with love; and the smallest amount of gross bodily movement occurred with hopelessness. The average scores were on the whole most extreme for anger, hunger, curiosity, love, amorousness, and passion; and the average scores were least extreme for sympathy, admiration, and tenderness.

It is important to note that the naming of the emotions and the measures of latent time and duration were based on the introspective reports of the S's, and that the breathing and movement scores were purely objective measures. When the results are calculated in this way for the different emotions, the greatest amount of variation occurs in the duration scores, with the latent time scores second, and the breathing scores third, and the least amount of variation occurs in the scores for gross bodily movement. We do not interpret this result, however, as indicating that the physiological activities in-

Emotion	Lat. time	Duration	Breathing	Movement
Horror	*short*	*long*	——	——
Fear	short	——	——	small
Disgust	short	long	——	——
Repulsion	*short*	*long*	——	——
Aversion	long	——	——	——
Dislike	*long*	*short*	——	——
Annoyance	long	*short*	——	——
Anger	*short*	*long*	large	——
Sadness	——	*long*	small	——
Sorrow	——	*long*	——	——
Hopelessness	long	——	——	*small*
Despair	——	long	small	——
Pity	——	——	——	small
Sympathy	——	——	——	——
Hunger	*short*	long	large	large
Interest	long	short	——	——
Curiosity	*long*	——	small	large
Pleasure	——	short	——	——
Aes. pleasure	——	——	small	——
Delight	——	——	——	large
Fascination	——	——	small	——
Admiration	——	——	——	——
Amusement	——	*short*	——	large
Humorousness	*short*	——	large	——
Tenderness	——	——	——	——
Affection	——	——	small	small
Love	*short*	*long*	*large*	——
Amorousness	*short*	*long*	large	——
Passion	short	*long*	large	——

volved in the emotions are unimportant, or that no better bodily criteria of emotions can be found than the breathing and gross bodily movement scores obtained in the present experiment.

In treating the data given in Table 4 it might be of some interest to arrange all of the emotions in fewer groups and to calculate the results for each group. The reliability of the average scores would be increased if this were done, but the meaning of the figures would not be as clear.

In Table 5 we have brought together the principal results on sex

TABLE 5

Sex Differences in the Feelings and Emotions Called Out by Different Pictures

(Abbreviations of emotions: *ad*, admiration; *ae*, aesthetic pleasure; *af*, affection; *amo*, amorousness; *amu*, amusement; *ang*, anger; *ann*, annoyance; *av*, aversion; *c*, curiosity; *de*, delight; *disg*, disgust; *disl*, dislike; *f*, fascination; *ho*, horror; *hu*, humorousness; *i*, interest; *l*, love; *pa*, passion; *pi*, pity; *pl*, pleasure; *r*, repulsion; *s*, sympathy; *t*, tenderness.)

Picture			Median feeling grade		Emotions			
					Men		Women	
Classification	Number	Content or subject-matter	Men	Women	No.	Names	No.	Names
War horrors	1	Living man's profile with nose and mouth shot away	—2.8	—2.8	12	2 ang, 2 disg, 5 ho, 2 r	13	7 ho, 4 r
	2	Group of seven soldiers with badly mutilated faces	—2.1	—2.0	13	6 r	10	2 av, 2 i, 3 r, 2 s
	3	Skeleton-like dead bodies of three starved children	—2.7	—2.9	8	2 disg, 2 ho, 2 r	10	5 ho, 5 r
	4	Dismembered corpse of soldier	—2.2	—2.9	12	2 ang, 2 disg, 3 ho, 2 r	12	5 ho, 3 r
	5	Two putrefying soldiers' corpses, one hanging in tree	—2.7	—2.8	11	2 ang, 2 disg, 2 ho, 2 r	13	5 ho, 5 r
	6	Group of putrefying soldiers' corpses	—2.6	—2.9	12	3 disg, 3 ho, 3 r	12	2 av, 2 disg, 4 ho, 2 r
	7	Roadside shrine in devastated war zone	—0.2	—1.0	7	2 ang, 2 i	10	2 c, 3 s, 3 i
	8	French execution of German army officer	+0.3	—0.8	9	2 ad, 2 amu, 2 ann	6	2 pi
	9	Manet's painting, "Execution" (colored)	—1.7	—2.2	9	2 disg, 2 pi	10	3 ho, 3 r

TABLE 5 (*continued*)

Classification	Number	Picture: Content or subject-matter	Median feeling grade: Men	Median feeling grade: Women	Emotions: Men No.	Emotions: Men Names	Emotions: Women No.	Emotions: Women Names
Mental deficiency	10	Two male syphilitic aments (from Tredgold)	—2.0	—2.8	9	2 disg, 2 ho	7	2 amu, 2 disg
	11	Two female Mongolian imbeciles (from Tredgold)	—2.5	—1.9	14	2 amu, 3 disg, 4 pi, 2 r	11	3 disl, 2 i, 3 r
	12	Two female imbeciles (from Tredgold)	—1.3	—1.7	12	3 disg, 2 i, 2 pi, 3 r	13	2 av, 3 c, 3 pi, 2 r
	13	Print of head with face scarred and with large cavity in parietal lobe	—2.6	—1.5	14	2 c, 2 disg, 4 r	10	3 c, 2 i, 4 r
Religion	14	Head of Jesus with blood on forehead (colored)	—1.2	—1.3	7	2 ann	7	2 pi, 2 s
	15	Mediaeval painting of Christ bleeding on cross (colored)	—1.7	—1.3	12	3 disl, 2 ho	8	2 disl, 3 pi
	16	Christ preaching the Sermon on the Mount	+0.7	+0.7	11	2 ad, 7 i	7	4 i
	17	Nun reading the Bible ("The Light")	+0.5	+1.9	6	2 i, 2 t	8	3 pl, 2 t
Handsome men	18	Ronald Colman	+1.5	+1.8	7	4 ad	7	
	19	Clark Gable	+0.7	+1.5	6	2 ad	8	2 ann
	20	David Manners	+0.3	+1.7	5	2 ad, 2 i	7	3 ad, 2 pl
	21	Ramon Novarro	+1.0	+2.0	7	2 ad	8	2 af, 2 pl
	22	Richard Cromwell	+0.7	+2.5	6	3 ad, 2 disl	8	2 amo, 2 pl
	23	Johnny Weissmuller dressed in loin cloth, as "Tarzan"	+1.7	+1.5	6	4 ad	6	4 ad

TABLE 5 (*continued*)

Classification	Number	Picture: Content or subject-matter	Median feeling grade: Men	Median feeling grade: Women	Emotions: Men: No.	Emotions: Men: Names	Emotions: Women: No.	Emotions: Women: Names
Beautiful women	24	Greta Garbo	+1.3	+1.8	11	4 ad, 2 disl	11	6 ad, 3 i
	25	Sylvia Sidney	+1.9	+2.0	13	2 ad, 3 amo, 4 f, 2 pa	10	5 ad, 2 pl
	26	Sidney Fox	+2.1	+2.1	10	3 l, 3 t	12	4 ad, 2 af
	27	Constance Cummings	+2.0	+1.7	8	3 af, 2 pa	5	4 ad
	28	Girl in bright red velvet dress	+2.4	+1.9	12	2 amo, 5 pa	10	2 ad
Suggestive pictures of women	29	Side view of blonde girl in short tights	+1.1	—0.5	10	7 pa	10	3 amu, 2 av, 4 disl
	30	Girl in tropical costume	+2.1	+2.4	13	2 amo, 3 de, 2 f, 3 pa, 2 pl	10	2 ae, 5 ad, 2 i
	31	Brunette girl dressed in two sheer scarfs	+1.6	—0.7	11	3 pa	10	6 disl
	32	Reclining blonde bathing beauty	+1.2	+1.5	12	5 pa, 2 pl	9	2 ae, 3 pl
	33	Faces of man and reclining woman about to kiss	+1.8	+1.9	9	2 l, 2 pl	12	2 af, 3 amo, 3 l
Babies	34	Dimpled baby sticking out tongue (colored)	+1.8	+2.5	13	2 af, 2 amu, 3 pl, 2 t	7	3 af, 2 t
	35	Sturdy naked baby boy	+1.3	+2.7	12	4 af, 2 pl, 6 t	9	2 l, 4 pl, 2 t
	36	Little girl holding baby brother (colored)	+1.5	+2.7	11	4 af, 2 pl, 2 t	10	2 ad, 2 af, 2 l, 3 t
	37	Comical baby about to touch his first birthday cake	+0.7	+1.9	10	3 af, 2 amu	11	2 af, 2 amu, 2 pl, 2 t
Mothers	38	Young mother in orange robe holding infant	+1.1	+2.3	9	5 t	12	8 t
	39	Elderly mother sewing by lamp light (colored)	+1.0	+1.8	12	2 amu, 4 t	12	2 ad, 3 af, 2 l, 2 t

TABLE 5 (*continued*)

Classification	Number	Picture: Content or subject-matter	Median feeling grade: Men	Median feeling grade: Women	Emotions: Men No.	Emotions: Men Names	Emotions: Women No.	Emotions: Women Names
Amusing and humorous pictures	40	Picture of clothed man taken with soles of bare feet greatly exaggerated in size by being near camera	+0.5	+0.6	11	3 amu, 2 disg, 5 hu	13	6 amu, 2 disl, 2 hu
	41	Dignified small man with unusually large moustache taking shower bath	+1.7	+1.0	12	4 amu, 5 hu	13	8 amu, 3 hu
	42	Elderly man in old-fashioned bathing suit searching for false teeth in ocean	+1.3	+0.7	15	9 amu, 5 hu	8	4 amu, 2 av, 2 hu
	43	Plump actress in old-fashioned tights taking sun bath in rays of kerosene lantern	+1.6	+1.1	13	6 amu, 2 disg, 2 disl, 3 hu	13	9 amu, 2 hu
	44	Ocean bathing scene of terrified fat woman standing on small husband in water	+1.8	+0.8	13	7 amu, 4 hu	12	4 amu, 2 disg, 2 disl, 4 hu
Scenic	45	Pine trees and fisherman on rocky shore	+1.5	+2.3	8	2 i, 3 pl	7	3 ae, 4 pl
	46	Placid lake with trees and high mountains in background	+0.9	+1.9	10	2 f, 3 i, 3 pl	9	4 f, 2 pl
	47	Snow scene on woodland road	+1.4	+2.3	13	2 ae, 3 de, 2 i, 5 pl	9	2 ad, 3 pl
	48	High cliff and distant panoramic view (colored)	+1.6	+2.0	11	2 ad, 2 de, 2 f, 3 i, 2 pl	7	4 i
	49	Placid lake showing reflection of mountains and sky (colored)	+2.0	+2.0	11	2 ae, 2 de, 2 f, 2 i	12	2 ad, 4 ae, 2 f, 3 pl

TABLE 5 (*continued*)

Classification	Number	Picture Content or subject-matter	Median feeling grade Men	Median feeling grade Women	Emotions Men No.	Emotions Men Names	Emotions Women No.	Emotions Women Names
Miscellaneous	50	Jar of flowers (colored)	+1.1	+2.0	15	2 ae, 3 de, 9 pl	10	3 ae, 2 f, 3 pl
	51	German painting of half-nude Cleopatra and the asp (colored)	+0.6	—0.5	13	3 disg, 2 disl, 2 pl	12	2 disl, 3 i, 2 r
	52	Grotesque modernistic painting of three nude men and two women	+0.9	—1.0	11	2 amu, 3c, 2 hu	14	2 amu, 2 c, 5 disl, 2 i
	53	Young housewife exhausted from cleaning, resting in chair (colored)	—0.1	—0.5	9	3 amu, 3 s	9	4 amu, 2 pi
	54	Pitiful homeless old women and child	—1.7	—1.2	10	9 pi	10	6 pi, 4 s
	55	Scene from moving picture, "Frankenstein"	—1.3	—2.0	11	2 ann, 2 hu, 3 ho	8	2 ho, 2 i, 3 pi
	56	Fierce Manchurian war lord (colored)	—0.3	—0.2	8	3 disl, 2 i	9	2 disl, 2 i
	57	Leopard pouncing on prey (colored)	—0.1	—0.5	5	2 c	10	3 f, 2 i
	58	Sleeping dog and two playful puppies (colored)	+2.0	+2.1	12	2 af, 6 amu, 2 pl	11	4 amu, 4 de
	59	Male abdominal cavity (colored)	—0.7	—0.5	9	6 i	13	3 c, 6 i, 2 r
	60	Two wrestlers in strenuous action	+1.0	—0.5	9	2 amu, 4 i	10	2 disg, 6 i

differences in the feelings and emotions that were called out by the 60 most frequently used pictures. These pictures have been classified, and renumbered consecutively. In the table the content of each picture is described as briefly and as objectively as possible. The median feeling scores are given for men and for women in the central portion of the table. The number of times the 23 most frequently reported emotions were evoked by each picture in both sexes is shown on the right. An alphabetical list of these emotions, together with the abbreviations used, is given at the head of the table. If an emotion was evoked by a particular picture less than two times, no record is made in the right-hand portion of the table. The number preceding each abbreviation on the right shows how many different men or women gave this particular emotion for the picture in question. The total number of emotions of all kinds called out by each picture in all of the men or women is shown, and these numbers are approximately the same as those on which the median feeling grades for men and women are based.

The first picture in the table is a war horror, a "Living man's profile with nose and mouth shot away." The median feeling grade was —2.8 for both men and women. Out of 12 emotions of all kinds called out by this picture in 12 different men, there were two cases of anger, two of disgust, five of horror, and two of repulsion. Out of 13 emotions of all kinds called out by the same picture in 13 different women, there were seven cases of horror and four of repulsion. Each of the S's had a list of 32 different emotions before him, and seven out of the 13 women S's reported the emotion of horror for this picture. The cases where the same emotion was given by only two men or by only two women are much less reliable than those in which the same emotion was given by three, four, or more men or women.

The feelings called out by the pictures of war horrors were quite unpleasant and approximately the same for both sexes, except with the picture of the French execution of a German army officer. Some of the men admired the brave bearing of the officer as he faced the firing squad. This group of pictures called out horror more frequently in women than in men. The three pictures from Tredgold's book on *Mental Deficiency* called out decidedly unpleasant feelings although approximately half of the S's were somewhat accustomed to them. For the picture of Christ preaching the Sermon on the Mount, 7 men out of 11 and 4 women out of 7 reported the "emotion" of interest.

The feelings and emotions evoked by the picture of moving-picture actresses and actors, like many other pictures, were definitely conditioned by the S's previous experiences. The feelings of the two sexes were equally pleasant for Ronald Colman and Johnny Weissmuller, but the women reacted more positively than the men to Clark Gable, David Manners, Ramon Novarro, and especially to Richard Cromwell. Two of the women reported affection for Ramon Novarro, and two women gave amorousness for Richard Cromwell. Admiration was the most common emotion experienced by the men. Although the pictures of handsome men were on the whole more pleasing to the women than to the men, the pictures of beautiful women, while quite pleasing to the men, were just as pleasing to the women. The most common emotion reported by the women was admiration, although two of them experienced affection for Sidney Fox. There were several cases of passion and amorousness among the men. Out of 10 emotions called out in the men by the picture of Sidney Fox, there were three of love and three of tenderness. The two most suggestive pictures of women were Nos. 29 and 31. Among the men there were 10 cases of passion, and among the women there were three cases of amusement, two of aversion, and 10 of dislike. For the women S's, the most objectionable feature of the two girls shown in these pictures was their faces, but the men did not seem to notice their faces. The pictures of the four babies and the two mothers produced pleasant feelings in both sexes, but the feelings were much more pleasant in the women. In the picture of the young mother dressed in an orange robe holding an infant, it is interesting to note that with a list of 32 different emotions before them, five out of nine men and eight out of 12 women reported the emotion of tenderness.

The five amusing and humorous pictures called out pleasant feelings in both sexes, but the feelings were much more pleasant in the men. Out of a total of 123 emotions called out by these five pictures in both sexes, there were 60 emotions of amusement and 35 of humorousness. The scenic pictures were pleasant for both sexes, but more pleasant for the women; and the "emotion" of pleasure was frequently reported by both sexes. Among the miscellaneous pictures in the last group, the jar of flowers was more pleasant for the women. The German painting of half-nude Cleopatra and the asp, the grotesque modernistic painting of three nude men and two women, and the picture of the two wrestlers in strenuous action were slightly pleasant for the men and slightly unpleasant for the women. The picture of the pitiful homeless old woman and child called out pity in nine out of 10 men and in six out of 10 women.

The data in Table 5 show that there was considerable correspondence between the feelings and emotions of the two sexes in their reactions to the different kinds of pictures, but the differences between the sexes that have been noted above also appear to be of general significance.

Summary and Conclusions

A study was made of the feelings and emotions which subjects experienced while they were looking at a number of pictures. Measures were obtained of the latent time and duration of the emotions, and of the amount of breathing and gross bodily movement which occurred while the emotions were present. Gross bodily movement was measured by a specially constructed chair connected with recording apparatus, and the total amount of breathing was measured by the number of electric contacts made by a belt apparatus during a given interval of time. Thirty-two subjects were used, and the experimental period with each subject lasted for two hours. The quantitative results on latent time, duration, breathing, and movements were converted into 10-percentile scores on the basis of the results for each S. A score of 10, for example, indicates that the measure was in the highest 10% of the time, duration, breathing, or movement scores of the subject, and a score of one indicates that the measure was in the lowest 10% of the time, duration, breathing, or movement scores of the subject. The principal conclusions which seem justified are as follows:

1. Under the conditions of the experiment, the speed and duration of the emotions are positively correlated with each other.
2. The breathing and gross bodily movements which occur while emotions are present are also positively correlated with each other.
3. The latent time of pleasant and unpleasant emotions is approximately the same.
4. The latent time of emotions is definitely faster when the emotions are quite pleasant or unpleasant than when they are indifferent or only slightly pleasant or unpleasant.
5. The duration of emotions is longer for unpleasant than for pleasant emotions.
6. The duration of emotions is definitely longer when the emo-

tions are quite pleasant or unpleasant than when they are indifferent or only slightly pleasant or unpleasant.

7. Approximately the same amount of breathing and gross bodily movement occurs during quite pleasant, indifferent, and quite unpleasant emotions. No reliable difference was found between the average breathing or movement scores for pleasant and unpleasant feelings or for weak and strong feelings.

8. Data are given on the average latent time, duration, breathing, and gross bodily movement scores for the 29 most frequently reported emotions. The greatest amount of variation occurred in the duration scores and the least amount in the scores for gross bodily movement.

9. Some results have also been given on sex differences in the feelings and emotions that were called out by the 60 most frequently used pictures. The results indicate that pictures constitute good material for investigations of this kind.

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L'AFFECTIVITÉ EN RELATION AVEC LA RESPIRATION ET LE MOUVEMENT GLOBAL DU CORPS

(Résumé)

On a fait une étude des sentiments et des émotions éprouvés par des sujets en regardant nombre de tableaux. On a obtenu des mesures du temps latent et de la durée des émotions, et de la quantité de la respiration et du mouvement global du corps qui a eu lieu quand les émotions ont été présentes. On a mesuré le mouvement global du corps au moyen d'une chaise spécialement construite jointe à un appareil enregistreur, et on a mesuré la quantité totale de respiration par le nombre de contacts électriques faits par un appareil de ceinture pendant un intervalle donné de temps. On s'est servi de 32 sujets, et la période expérimentale pour chaque sujet a duré deux heures. Les résultats quantitatifs du temps latent, de la durée, de la respiration, et du mouvement ont été convertis en résultats 10-percentiles sur la base des résultats pour chaque sujet. Un résultat de 10, par exemple, indique que la mesure a été dans le 10 pour cent le plus

élevé des résultats de temps, de durée, de respiration, ou de mouvement du sujet, et un résultat de 1 indique que la mesure a été dans le 10 pour cent le moins élevé des résultats de temps, de durée, de respiration, ou de mouvement du sujet. Les principales conclusions qui semblent justifiées sont les suivantes:

1. Dans les conditions de l'expérience, la vitesse et la durée des émotions donnent une corrélation positive l'une avec l'autre.
2. La respiration et les mouvements globaux du corps qui ont lieu pendant que les émotions sont présentes donnent aussi une corrélation positive l'une avec l'autre.
3. Le temps latent des émotions agréables et désagréables est approximativement le même.
4. Le temps latent des émotions est définiment plus rapide quand les émotions sont tout à fait agréables ou désagréables que quand elles sont indifférentes ou seulement un peu agréables ou désagréables.
5. La durée des émotions est plus longue pour les émotions désagréables que pour les agréables.
6. La durée des émotions est définiment plus longue quand les émotions sont tout à fait agréables ou désagréables que quand elles sont indifférentes ou seulement un peu agréables ou désagréables.
7. Approximativement la même quantité de respiration et de mouvement global du corps a lieu pendant les émotions très agréables, indifférentes, et très désagréables. On n'a trouvé aucune différence constante entre les résultats moyens de respiration ou de mouvement pour les sentiments agréables et désagréables ni pour les sentiments faibles et forts.
8. On offre des données sur les résultats moyens de temps latent, de durée, de respiration, et de mouvement global du corps pour les 29 émotions le plus fréquemment rapportées. La plus grande quantité de variation a eu lieu dans les résultats de durée et la plus petite quantité dans les résultats du mouvement global du corps.
9. On a donné aussi quelques résultats sur les différences de sexe dans les sentiments et les émotions évoqués par les 60 tableaux le plus fréquemment employés. Les résultats indiquent que les tableaux sont un bon matériel pour des investigations de cette sorte.

CASON ET CASON

DIE AFFEKTIVITÄT IN IHRER BEZIEHUNG ZUR ATMUNG UND ZU DEN GROSSEN KÖRPERLICHEN BEWEGUNGEN

(Referat)

Man untersuchte die von Versuchspersonen während der Betrachtung einer Anzahl Bilder empfundenen Gefühle und Gemütsbewegungen. Man machte Messungen der Latenzzeit und der Dauer der Gemütsbewegungen und der Zahl der Respirationen und der grossen Körperbewegungen, die während des Bestehens der Gemütsbewegungen stattfanden. Die grossen Körperbewegungen wurden mittels eines besonders konstruirten, mit einem Registrierungsapparat verbundenen Stuhls gemessen, und die Gesamtzahl der Respirationen wurde an der Anzahl der mit einem Gürtelapparat [belt apparatus] im Laufe eines gewissen Zeitintervalles etablierten Kontakte gemessen. Es wurden 32 Versuchspersonen verwendet, und die Experimentierungsperiode war bei jeder Vp. zwei Stunden. Die quantitativen Befunde in Bezug auf die Latenzzeit, Dauer, Respiration, und Bewegung

wurden, auf Basis der Resultat mit jeder Vp., in zehn-prozentige Vergleichungszahlen [10-percentile scores] umgearbeitet. Die Zahl 10, zum Beispiel, wies darauf hin, dass die betreffende Messung in den höchsten Zehn-Prozent der in Bezug auf Latenzzeit, Dauer, Respiration, oder Bewegung von der Vp. erreichten Zahlen gehörte. Die wesentlichen, berechtigt erscheinenden Schlüsse sind folgende:

1. Unter den Bedingungen des Versuches besteht eine positive Korrelation zwischen der Schnelligkeit [speed] und der Dauer der Gemütsbewegungen.
2. Zwischen der Respirationszahl und den während des Bestehens der Gemütsbewegungen stattfindenen grossen Körperbewegungen besteht ebenfalls eine positive Korrelation.
3. Die Latenzzeit angenehmer und unangenehmer Gemütsbewegungen ist ungefähr die selbe.
4. Die Latenzzeit der Gemütsbewegungen ist bestimmt geringer, wenn die Gemütsbewegungen entweder recht angenehm oder recht unangenehm sind, als wenn sie gleichgültig oder nur etwas angenehm oder unangenehm sind.
5. Die Dauer der Gemütsbewegung ist bei unangenehmen Gemütsbewegungen länger, als bei angenehmen.
6. Die Dauer der Gemütsbewegungen ist bestimmt länger, wenn letztere recht angenehm oder recht unangenehm sind, als wenn sie gleichgültig oder nur etwas angenehm oder unangenehm sind.
7. Der Betrag an Respirationen und an grossen Körperbewegungen ist während recht angenehmen, gleichgültigen, und recht unangenehmen Gemütsbewegungen ungefähr der selbe. Es zeigte sich kein zuverlässiger Unterschied zwischen den durchschnittlichen Respirationszahlen oder Bewegungszahlen für angenehme bzwl. unangenehme noch für schwache bzwl. starke Gemütsbewegungen.
8. Es werden statistische Befunde mittgeteilt über die durchschnittliche Latenzzeit, Dauer, Respiration, und grossen Körperbewegungen bei den 29 am häufigsten gemeldeten Gemütsbewegungen. Die grössten Abweichungen fand man in den Dauerzahlen und die geringsten in dem Betrag an grossen Körperbewegungen.
9. Es werden auch einige Befunde mitgeteilt über Geschlechtsunterschiede in Bezug auf die durch die 60 am häufigsten verwendeten Bilder hervorgerufenen Gefühle und Gemütsbewegungen. Die Befunde weisen darauf hin, dass Bilder für Untersuchungen dieser Art einen guten Stoff bilden.

Cason und Cason

THE RELATIVE SUSCEPTIBILITY OF PLEASANT, UNPLEASANT, AND INDIFFERENT MATERIAL TO RETROACTIVE INHIBITION*

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Introduction and Problem

Some experimental work in the field of memory indicates that the amount of retroactive inhibition may vary with the affective nature of the material learned. Tolman (6) found less interference from interpolation when the original learning material was pleasant words than when the learning material consisted of indifferent words. This study is one of the few dealing directly with the relation of the affective nature of the material to retroaction. The experimental work concerning the relation of feeling to memory has been reviewed by Meltzer (4). Another aspect of the problem, namely, the relation of the affective nature of the interpolated material to the amount of retroactive inhibition, has been studied by Frank and Ludvigh (2). These investigators found that recall of nonsense syllables after a series of indifferent odors was more efficient than when such recall followed a series of unpleasant odors, but less efficient than when the interpolated series of odors was pleasant.

The present study was conducted to determine the relative susceptibility of pleasant, unpleasant, and indifferent memory material to the retroactive inhibition resulting from the mastery of indifferent material during the interval before retention, when the affective material for a given subject is chosen on the basis of this subject's reactions. Usually the affective value of the words or material to be learned has been determined by the experimenter with the aid of a few judges. It is highly probable that a list of words judged to be pleasant, for example, by a few individuals, may contain some indifferent words and a few actually unpleasant to other subjects. When the variable under investigation is the affective nature of

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the material, then probably it is better procedure to have the affective material for a given subject chosen on the basis of this subject's reactions.

Conditions of the Experiment

One hundred and twenty-five students, 60 women and 65 men, served as subjects in this experiment and were divided into six groups (three work and three rest) on the basis of the six different conditions under which learning and retention were measured. Groups P, I, and U learned pleasant, indifferent, and unpleasant material respectively under normal conditions, and were tested for retention two days later. No control was exercised over the activities of the individuals in these groups during the interval: They went about their affairs as though they were not taking part in an experiment. Each of these groups is the control or standard group for the one of the following three groups which has the same letter.

Groups P_1, I_1, and U_1 also learned pleasant, indifferent, and unpleasant material and recalled the material after an interval of 48 hours. Each of these groups, however, mastered a second problem, consisting of indifferent material, at the end of the 48-hour interval and just prior to the retention test of the original material.

The retention data of Groups P, I, and U indicate the amount of pleasant, indifferent, and unpleasant material normally retained over an interval of 48 hours. The differences in retention between each of the groups, P_1, I_1, and U_1, and its corresponding letter group in the above control series, will indicate the effect of the interpolated indifferent material upon the retention of material of different affective value.

The learning material for each individual was a list of 15 words, a separate list being prepared for each subject. For those individuals in the groups which had a second problem to master before the relearning of the first, a second list was prepared. For instance, a separate list of 15 pleasant words was prepared for each individual in the P group, a separate list of unpleasant words for each subject in the U group, and so on. The particular words constituting the problem for each person were chosen from a larger list of words, in a preliminary study, on the basis of the individual's affective reaction to each item in the list. A description of the method employed in choosing the affective words follows.

Prior to the main experimental work reactions of each of 143 subjects were obtained to a list of 100 words presented singly. This list contained about an equal number of words generally regarded as pleasant, indifferent, and unpleasant, and was prepared on the basis of such studies as word association tests, tests for investigating the emotions, and the like, reported in the literature concerning affective material (3, 5). The affective value of each of the 100 words in the list was determined for each individual on the basis of (1) his judgment as to whether it was pleasant, unpleasant, or indifferent, and (2) the accompanying physiological change, as registered by a galvanometer, which occurred in response to each word.

This variation in experimental procedure in regard to selection of materials of different affective value has been used in an earlier unpublished study from this laboratory, "The Memory Value of Affectively Toned Words" by V. W. Lemmon and M. N. LaSater. The reasons for the modification in procedure are clearly stated in the following quotation from this earlier investigation:

> "In those experiments on the influence of affective tone on memory in which discrete words were used as material, it has been the general practice to use the same list of words for each subject. No doubt this was for the purpose of 'keeping conditions constant.' It is evident from our preliminary work, that it is practically impossible to find a list of words which will have the same affective tone for any considerable number of subjects. Some of the words which are pleasant, for example, for some subjects, are almost certain to be indifferent or even unpleasant for other subjects. In our opinion it is more important to attempt to keep the affective tone constant than to use the same words for each subject. Accordingly each subject had a different list of words, but those in the 'pleasant' group, for example, were in each case those which were really pleasant for the subject in question.
>
> "The second modification in technique concerns the method used to determine the affective tone of the words. For each word in the original list, an introspective report and a galvanometer reading were taken. In making up the pleasant list, for example, only those words were taken which were introspectively reported as pleasant, and which also gave a galvanometer deflection. If any word did not satisfy *both* these criteria, it was not included in the final list. The writers are aware of the doubt regarding the real meaning of the psychogalvanic reflex; nevertheless it seems probable that a word

> which is reported as pleasant and also causes a deflection, is more likely to be really pleasant than if the introspective criterion alone is satisfied."

The procedure for one individual is described. When the subject reported in the laboratory for this part of the experiment he was seated in a comfortable chair and connected to a standard Hathaway galvanometer by strapping the two small electrodes on the palm of the left hand. The subject was told that a number of words were to be presented, one at a time, and that in response to each he was to give first, the first association that occurred to him and second, his judgment as to whether the word presented was pleasant, unpleasant, or indifferent. The purpose of having the subject give the first word suggested to him by the stimulus word was mainly to insure an active attitude on the part of the individual and a definite reaction to the word presented. Opposite each stimulus word in the list the experimenter recorded, in addition to the subject's response word, the judgment of the type of affectivity and the galvanometric deflection. The 100 words used in the list are presented in Table 2.

After the relative affective values of the 100 words had been determined for the 143 individuals taking part in the preliminary work, the particular words to be learned later by each individual were selected on the basis of the two criteria of affectively employed: the verbal report of the subject, and the galvanometric deflection. For example, in preparing the pleasant material, the 15 words chosen for each individual were those with the greatest galvanometric excursion which the subject had judged to be pleasant. The unpleasant words chosen were those with the greatest deflection which were judged to be unpleasant. For the indifferent lists those words were selected which were followed by the least deflection and which were judged by the individual to be lacking in affective value. In Table 2 opposite each of the 100 words is given the number of pleasant, unpleasant, and indifferent lists, respectively, in which that particular word appeared on the basis of the final selection. For instance, the second word in the table is *spit* and this word did not meet the criteria for a pleasant or an indifferent word for a single subject but did meet the criteria as an unpleasant word for 28 individuals, and hence appeared in the list of unpleasant words for each of these individuals. Of the 100 words 53 were used in at least one pleasant list, 63 were used in at least one unpleasant

TABLE 1

PROGRAM OF THE CONDITIONS OF LEARNING FOR EACH GROUP

Group	No. of S's	Original learning problem	Interval of 48 hours	Immediately preceding retention test	Retention test
P	20	Pleasant words	*Normal activity		Relearn. prob. 1
P_1	20	" "	Normal activity	Indifferent words	" " "
U	21	Unpleasant words	Normal activity		" " "
U_1	21	" "	Normal activity	Indifferent words	" " "
I	21	Indifferent words	Normal activity		" " "
I_1	22	" "	Normal activity	Indifferent words	" " "

*By normal activity is meant that the individuals went about their affairs as if they were not taking part in an experiment.

TABLE 2

List of Words Used in the Present Experiment

	Pl.	Unpl.	Ind.		Pl.	Unpl.	Ind.
Word	0	1	46	Theatre	20	0	9
Spit	0	28	0	Carrot	3	4	30
Cow	5	1	39	Disgust	0	18	0
Music	19	2	8	Kiss	29	1	0
Disgrace	0	30	0	Chair	0	0	56
Sunshine	21	0	8	Nausea	0	23	0
Mountains	20	1	21	Box	2	0	42
Pencil	0	3	47	Suck	2	4	1
Sewer	0	18	8	Rotting	0	11	7
Glass	0	2	42	Love	39	2	8
Filthy	0	14	0	Money	17	1	17
Flowers	22	0	11	Honor	18	0	4
Cruel	0	11	4	Cheat	0	21	3
Desk	2	1	47	Bury	1	5	8
Tennis	20	0	15	Girl	22	0	7
Drunk	3	21	0	Walk	2	1	25
Street	1	0	50	Kill	0	17	2
Shut	0	1	62	Swim	8	1	8
Laugh	20	0	26	Radiator	2	1	37
Mother	23	0	2	Dance	20	1	3
Cement	1	0	60	Indecent	1	24	1
Illegitimate	0	22	1	Worm	0	10	23
Cook	7	1	33	Door	0	0	52
Abortion	0	20	0	Divorce	0	4	14
Slash	0	8	5	Ugly	0	12	3
Travel	14	0	13	Assault	0	17	4
Puss	4	6	17	Table	2	0	55
Dead	0	11	4	Afraid	0	20	4
Bag	2	0	48	Choice	6	0	37
Sleep	14	0	8	Wicked	0	10	3
Insult	0	18	1	Sweet	15	0	6
Needle	1	3	47	Wall	0	0	50
Despise	0	10	1	Pain	0	8	1
Win	11	0	8	Friend	20	0	1
Poison	0	13	1	Vomit	0	21	0
Streetwalker	0	15	0	Dirty	0	5	4
Bitter	0	4	18	Food	10	0	15
Hunger	0	8	8	Thief	0	7	7
Aroma	10	0	14	Home	17	0	5
Sin	0	20	1	Bored*			
Floor	0	0	51	Marry	15	0	2
Luck	13	0	10	Glove	1	1	30
Pox	0	24	5	Vacation	14	0	5
Slime	0	5	2	Snake	0	11	7
Breeze	13	0	19	Succeed	21	0	6
Sad	0	10	7	Log	0	0	30
White	3	0	46	Meanness	0	14	6
Village	3	0	49	Summer	10	0	3
Family	8	0	20	Contempt	0	20	2
Boat	9	1	24	Freedom	14	0	8

*This word was not used in any of the lists because in the preliminary work (oral presentation) it was frequently confused with the word *board*.

list, and 91 in at least one indifferent list. Of the 63 words which were used in making up the unpleasant lists, 33% were used at least once in pleasant lists.

An exposure apparatus, similar to the type ordinarily used in memory experiments in the laboratory, was employed in presenting the words to the subject during the learning and the test for retention. Each list was typewritten and the words were presented singly through the small aperture in the exposure apparatus. Each word was exposed for two seconds, the exposure rate being timed by a small light, visible to the experimenter but not to the subject, which flashed in time with a metronome in another room.

The material was learned by the anticipation method, that is, after exposing the words in succession once the subject was required to anticipate and give the word next in the list just before it appeared. Failure to anticipate, or responding with the wrong word, was counted as an error. Record was kept of the number of errors made and of the number of trials required to learn the list to the criterion of two errorless trials in succession.

Forty-eight hours after the mastery of the first problem the individuals returned to the laboratory for the second part of the experiment. At this time they relearned the original problem or mastered the interpolated indifferent material just prior to the relearning of the original problem, depending upon the group in which they had been placed.

Results

1. *The Relation between Learning Efficiency and the Affective Nature of the Material.* Of the 125 subjects who completed the experiment, 40 learned pleasant words (Groups P and P_1), 42 learned unpleasant words (Groups U and U_1), and 43 learned indifferent words (Groups I and I_1). A comparison of the records made in mastering these three classes of words will show the relation, under the conditions described above, between the affective nature of the material and the difficulty encountered in learning it. In Table 3 are given the averages of the learning criteria or measures, their differences, and the sigmas of the differences, for the pleasant, unpleasant, and the indifferent material.

It is evident from these data that learning efficiency varied with the affective character of the material. In terms of the averages, the individuals required fewer trials and made fewer errors to mas-

TABLE 3

ORIGINAL LEARNING DATA FOR PLEASANT, UNPLEASANT, AND INDIFFERENT MATERIAL

Groups	No. of S's	Material	Trials Av.	Trials $\sigma_{av.}$	Errors Av.	Errors $\sigma_{av.}$	Overlearning ratio av.
P plus P_1	40	Pleasant	22.07	1.65	119.00	12.38	193.97
U " U_1	42	Unpleasant	25.30	1.38	143.76	10.66	173.46
I " I_1	43	Indifferent	24.53	1.57	130.34	11.82	178.76

Differences

Groups compared	Av.	$\sigma_{diff.}$	Ratio	Av.	$\sigma_{diff.}$	Ratio	Av.
I plus I_1 — P plus P_1	2.46	2.27	1.08	11.34	17.09	.66	—15.21
I plus I_1 — U plus U_1	—.77	2.08	.37	—13.42	15.91	.84	5.30
Percentage decrease of P under I	10.02			8.70			
Percentage decrease of U under I	—3.14			—10.22			

ter the pleasant words than either of the other kinds of affective materials. In terms of both trials and errors, the unpleasant material was the most difficult of the three to learn. A comparison of the pleasant and the unpleasant material, with the indifferent material as a standard, indicates that the pleasant words were learned in 10.02% fewer trials and with 8.7% fewer errors, while the individuals learning the unpleasant material required 3.14% greater number of trials and made 10.22% greater number of errors than was required in mastering the indifferent material. In general, for most of the measures the chances are about 75 in 100 that with repetition of the experiment pleasant material would be more readily, and unpleasant material less readily, learned than indifferent material.

Another comparison is possible in terms of the overlearning ratio, the formula of which is $\frac{\text{correct anticipation}}{\text{failure to anticipate}} \times 100$. These ratios are also stated in Table 3. In general, in the instance of the larger overlearning ratios the subject presumably would have reached the criterion of mastery in fewer trials except for the unusual difficulty of a few items in the list. In regard to the three types of affective material studied, the pleasant words had the largest overlearning ratio, indifferent material next, with the unpleasant words being the least overlearned. Apparently the average number of trials and errors for the pleasant material, even though less than the corresponding scores for the other two materials, would have been slightly smaller, indicating greater ease in learning, had it not been for a few words offering greater difficulty than the majority of the words in this list.

2. *The Relation of the Affective Nature of the Learning Material to the Amount Retained for a Period of 48 Hours.* The relative amount of pleasant, unpleasant, and indifferent material retained after a lapse of 48 hours is obtained by comparing the retention scores of Groups P, I, and U with each other. These three groups learned and relearned their problems under the same conditions and with the same time interval separating the two tests, the essential difference between them being in the affective character of the material. The retention data are presented in Table 4. The groups are compared in terms of relearning, recall, and percentage saving scores. Relearning scores represent the number of trials and errors required by an individual at the

TABLE 4

RETENTION SCORES FOR PLEASANT, UNPLEASANT, AND INDIFFERENT MATERIAL

		Relearning Problem 3				Recall		Percentage savings	
		Trials		Errors		Errors		Trials	Errors
Group	Material	Av.	$\sigma_{av.}$	Av.	$\sigma_{av.}$	Av.	$\sigma_{av.}$	Av.	Av.
P	Pleasant	5.85	.75	8.80	2.99	2.50	.47	72.33	94.09
U	Unpleasant	7.19	.85	12.42	2.50	4.47	.69	73.55	90.62
I	Indifferent	5.14	.63	8.81	1.86	2.19	.46	73.28	92.71

TABLE 5

GROUP DIFFERENCES IN RETENTION

	Relearning Problem 1						Recall			Percentage savings	
	Trials			Errors			Errors			Trials	Errors
Groups compared	Av.	$\sigma_{diff.}$	Ratio	Av.	$\sigma_{diff.}$	Ratio	Av.	$\sigma_{diff.}$	Ratio	Av.	Av.
P — I	—.71	.97	.94	.01	3.51	.003	—.31	.65	.48	.95	—1.38
U — I	—2.05	1.07	1.92	—3.62	3.11	1.16	—2.28	.82	2.78	—.27	—2.09
Percentage increase of P over I	—13.81			.11			—14.11				
Percentage increase of U over I	—39.88			—41.08			—104.11				

end of the time interval to learn the problem to the same criterion of mastery employed in original learning. The number of errors made on the first trial in relearning represents the recall error score. Percentage saving scores were obtained by dividing the amount retained (the difference between learning and relearning) by the original learning score.

An inspection of recall scores indicates that the effectiveness with which the material was retained varies with the affective nature of the material. The average number of errors made in recalling the unpleasant words was 4.47, as compared to 2.50 and 2.19 errors for the pleasant and indifferent lists respectively. If we compare the pleasant and unpleasant groups to the indifferent group as a standard, it is found that pleasant words show only a slight decrease in the number retained whereas the unpleasant words show a greater and a significant decrease in the number of words retained. More than twice as many errors occur in the recall of the unpleasant list, compared to indifferent words, and the difference between the groups in this respect is 2.78 times the sigma of the difference. (See Table 5.)

A comparison of retention of the three types of affective material in terms of relearning scores gives the same general results which were found in recall scores: poorest retention for unpleasant words, and slight differences between the retention of the pleasant and the indifferent. For instance, in relearning the material to the same criterion employed in original learning the individuals required 7.19 trials for the unpleasant, 5.85 and 5.14 trials for the pleasant and indifferent words respectively. The number of errors made in relearning was 8.80 and 8.81 for the pleasant and the indifferent words as compared to 12.42 for the unpleasant words. That there was greater forgetting of the unpleasant as compared to the neutral material is indicated clearly by the following differences: the former required 39.88% more trials and made 41.08% more errors than the latter to reach the criterion of mastery in relearning.

The amount retained on the basis of percentage saving scores in terms of trials does not seem to vary with the affective character of the material; on the basis of percentage saving scores for errors the materials are ranked in decreasing order of efficiency as follows: pleasant, indifferent, and unpleasant. The differences, however, are slight.

3. *The Relation of the Affective Nature of the Material to the*

Amount of Retroactive Inhibition. The retention scores made by the individuals in Groups P, I, and U (for pleasant, indifferent, and unpleasant material respectively) show the amount of such material normally retained over an interval of 48 hours.

The relative susceptibility of material of different affective value to retroactive inhibition, which results from the mastery of a second problem during the interval between learning and retention, is obtained by comparing the retention scores of a group which learned an interpolated problem with the amount retained under normal conditions for that time interval. Groups P_1, I_1, and U_1 learned pleasant, indifferent, and unpleasant material respectively, and relearned this material after an interval of 48 hours. These groups differed from the corresponding letter Groups P, I, and U only in the respect that a second problem was learned just prior to the relearning of the first problem. The interpolated problem was, in every case, a list of indifferent words prepared for each subject in the manner already described.

a. The susceptibility of indifferent material to the detrimental effect of the interpolated problem. The influence of the interpolated indifferent material upon the retention of the first-learned indifferent words is obtained by a comparison of the recall, relearning, and saving scores of Group I with those of I_1. The amount retained under these two conditions is shown in Table 5. Comparative retention data are presented in Table 6. These data show that the acquisition of the second material during the interval interfered with the retention of the first problem by all criteria employed in measuring retention. The effect of learning the interpolated problem upon the retention of the previously learned indifferent material was an increase of 37.94% in the number of trials required, an increase of 118.41% in the number of errors made, in relearning; in terms of recall scores errors, an increase of 277.62% in the number of errors made; and, in regard to saving scores, a 3.58% less saving in the number of trials, and an 8.46% less saving in the number of errors. Thus a comparison of the retention scores for the two groups indicates that the interpolated problem had upon the retention of the first problem a slightly detrimental effect in terms of errors of recall, and errors and trials of relearning. The differences between the two groups in relearning and recall scores have, statistically, a high degree of reliability as indicated by the ratio of each difference to its sigma (Table 7).

TABLE 6
RETENTION SCORES FOR PLEASANT, UNPLEASANT, AND INDIFFERENT MATERIAL

		Relearning Problem 1				Recall		Percentage savings	
		Trials		Errors		Errors		Trials	Errors
Group	Material	Av.	$\sigma_{av.}$	Av.	$\sigma_{av.}$	Av.	$\sigma_{av.}$	Av.	Av.
P	pleasant	5.85	.75	8.80	2.99	2.50	.47	72.33	94.09
P_1	"	5.45	.64	10.90	2.28	5.05	.83	72.73	88.74
U	unpleasant	7.19	.85	12.42	2.50	4.47	.69	73.55	90.62
U_1	"	8.04	1.03	25.38	4.53	7.23	1.02	60.19	74.03
I	indifferent	5.14	.63	8.80	1.86	2.19	.46	73.28	92.71
I_1	"	7.09	.73	19.22	2.86	8.27	.85	69.70	84.25

TABLE 7

GROUPS COMPARED IN REGARD TO RETROACTIVE INHIBITION

Groups compared	Material	Relearning Problem 1						Recall Errors			Percentage savings	
		Trials			Errors						Trials	Errors
		Av.	$\sigma_{diff.}$	Ratio	Av.	$\sigma_{diff.}$	Ratio	Av.	$\sigma_{diff.}$	Ratio	Av.	Av.
P_1 — P	pleasant	—.40	.98	.41	2.10	3.76	.56	2.55	.95	2.68	.40	—5.35
U_1 — U	unpleasant	.85	1.33	.64	12.96	5.17	2.51	2.76	1.23	2.24	—13.36	—16.59
I_1 — I	indifferent	1.95	.96	2.03	10.42	3.51	3.05	6.08	.97	6.27	—3.58	—8.46
Percentage decrease of P under P_1		—6.84			23.86			102.00				
Percentage decrease of U under U_1		11.82			104.35			61.74				
Percentage decrease of I under I_1		37.94			118.41			277.62				

b. The susceptibility of pleasant material to the interference effect of the interpolated problem. The influence of indifferent material, interpolated during the interim, upon the retention of previously learned pleasant material is found by comparing the recall, relearning, and saving scores of Group P with those of P_1. The scores made on the retention test are presented in Table 6, and the comparative data are given in Table 7. Inspection of these data indicates that the interpolated indifferent material interfered with the retention of the pleasant words by all criteria except trials of relearning and the saving score for trials, in which a slight facilitation occurred. The effect of the interpolated material was an increase of 23.86% and 102.00% in terms of errors of relearning and errors of recall respectively, and a 5.35% less saving in terms of errors. Statistical measures of reliability indicate that the differences between the two groups in retention scores are of low reliability except in terms of errors of recall which represents a rather significant difference (chances being about 99 out of 100 that the direction of the difference would be confirmed by subsequent work). Thus, mastering a list of indifferent words during the interval between the learning and relearning of pleasant words influences detrimentally the retention of the pleasant material in terms of error scores, but has a facilitating effect upon retention when the latter is measured in terms of trials.

c. The susceptibility of unpleasant words to the detrimental effect of the interpolated problem. The effect upon retention of learning indifferent material prior to the relearning of the original unpleasant material is obtained by comparing the retention values of Group U with those of U_1. The amount retained under these two conditions is shown in Table 6, and the retention values are compared in Table 7. The interpolated problem of indifferent material has a detrimental effect upon the retention of the unpleasant words according to all criteria of measurement employed. The effect of the indifferent material upon the retention of the unpleasant material was an increase of 11.82% in the number of trials required, and an increase of 104.35% in the number of errors made, in relearning; an increase of 61.74% in the number of errors made in recall; in terms of saving scores, a 16.59% less saving in errors and a 13.36% less saving in the number of trials required. Thus, according to all measures used the results are consistent in indicating retroactive inhibition, in the retention of unpleasant material, from interpolated indifferent words. The retroactive effect was greatest in terms of the various error scores, and, statistically, these differences between the two groups have a fairly high degree of reliability (the ratios in Table 7 indicate that the chances are about 99 out of 100 that the differences on the basis of relearning and recall errors would be confirmed as to direction by subsequent work).

d. The relative susceptibility of pleasant, unpleasant, and indifferent material to the interference effect of an indifferent interpolated problem. In *a, b,* and *c* above it was found that pleasant, unpleasant, and indifferent materials are less effectively retained, for an interval of 48 hours, under the conditions of an interpolated indifferent problem than under normal conditions with no such interpolation. The question now arises, does the amount of retroactive inhibition, resulting from the acquisition of an indifferent problem during the interval, vary with the affective nature of the material learned originally? The differences in retention scores which occurred in the conditions $P\text{-}P_1$, $I\text{-}I_1$, and $U\text{-}U_1$, are to be compared with each other, since these conditions show the amount of retroactive inhibition of indifferent material upon pleasant, unpleasant, and indifferent material respectively. The data are shown in Table 7. The relative effect of the interpolated problem upon the retention of the three varieties of affective material was, in terms of relearning trials, a facilitating effect of 6.84% for pleasant words, a detrimental effect of 11.82% and a detrimental effect of 37.94% for the unpleasant and indifferent problems respectively; a detrimental effect in relearning errors of 23.86% for pleasant words, 104.35% for unpleasant words, and 118.41% for indifferent material; an increase in recall errors of 102.00%, 61.74%, and 277.62% for pleasant, unpleasant, and indifferent material respectively. That the amount of retroactive inhibition resulting from an interpolated indifferent problem varies with the affective nature of the material is clearly indicated in these comparisons. In general, the relearning and recall scores indicate that the amount of retroactive inhibition was greatest for indifferent material, next for unpleasant and pleasant in the order named. The relative amount of interference effect found in a comparison of the saving scores also indicates that the degree of retroaction varies with the affective nature of the original material. The decrease in the percentage saving was greatest for unpleasant words, next for indifferent material, with the pleasant words showing the least detrimental effect.

Discussion of the Results

Under the conditions of this experiment, pleasant material, on the average, was learned quicker, was retained better under normal conditions, and showed less susceptibility to the retroactive effect of interpolated indifferent material, than unpleasant material. Indifferent material occupied an intermediate position on the basis of original learning scores, was retained under normal conditions about as effectively as pleasant material, and showed greater susceptibility to retroactive inhibition in terms of relearning and recall scores, although less on the basis of percentage saving scores, than unpleasant material.

In attempting to explain these differences in the efficiency of retention of the materials of different affective value several factors which pertain to the conditions under which such differences were obtained are of importance:

1. *Similarity of Interpolated to Original Material.* The material interpolated during the interval between the original learning and the retention test was, in every case, indifferent material. The degree of similarity between the two learning situations is probably slightly greater when the material in the interpolated learning has been chosen from the same category as the first, with respect to its affective value, than when chosen from a different category. Hence, on the basis of slightly greater similarity, indifferent material might be expected to have a greater detrimental effect upon the retention of indifferent material than upon pleasant or unpleasant material. The results, however, are not consistent in indicating greater retroaction when the original material is indifferent than when pleasant or unpleasant; and the similarity factor could hardly account for the differences between pleasant and unpleasant material in the susceptibility to retroaction.

2. *The Degree of Learning Present Prior to the Test.* Possible differences between the materials in the degree of prior learning has been suggested in explanation of differences in the retention of pleasant, indifferent, and unpleasant material. The scores in the original learning test indicate that the pleasant material was the least difficult to learn. This fact might be interpreted as indicating that the pleasant material prior to the learning test was known better to the subjects than the indifferent and the unpleasant material. However, it is to be remembered that all words used in this experiment were well known to the subjects and inequalities in the degree of familiar-

ity with the material would probably be of less significance in this case because of the great amount of overlearning. The essential problem in the learning test was the memorization of the words in a particular series, and it is unlikely that any particular series used was favored greatly over any other on the basis of prior learning, and especially unlikely that all series of pleasant words were favored in this respect over the series of indifferent and unpleasant words.

3. *Overlearning.* This factor of overlearning is obviously related to the one discussed in the immediately preceding section. If the pleasant lists were known prior to the learning test better than the others, then after the learning test they were probably more overlearned than the others. There is some evidence, however, that in the learning test itself the pleasant lists were more overlearned than the others. As shown in Table 3, the pleasant words had the largest overlearning ratio, indifferent material next, and the unpleasant words were the least overlearned, although the difference between the indifferent and the unpleasant material in this respect was small. There is not a very consistent relationship between the overlearning ratios and the efficiency of retention under either normal or interpolated conditions of retention for the affective materials.

The results obtained in this experiment concerning the relationship between the affective nature of the material learned and susceptibility to retroactive inhibition can be stated readily enough in terms of the judgmental conception of affection advocated by Carr (1, pp. 290 ff.). In terms of this conception, the material which is judged as pleasant is the material to which normally a positive reaction is made, and the material labelled unpleasant is that to which normally a negative reaction is made. With respect to the results of the experiment then, it would seem that affective material toward which a positive response is usually made is learned quicker, retained better under normal conditions, and less susceptible to retroaction from interpolated indifferent material, than affective material toward which a negative response is made.

The results, however, are not as consistent with ordinary psychoanalytic interpretation, although greater retroactive inhibition occurred in the retention of the unpleasant than in the retention of either indifferent or pleasant material. The "inhibition of the unpleasant" in psychoanalytic terminology is the outcome of a conflict between the unpleasant material or experience and another experience which is relatively stronger or of greater emotional or affective value. The interference effect in the retention of materials of different affective value that occurred in this experiment, in so far as the control groups are adequate controls, occurred as the result of the interpolated *indifferent* material.

Summary and Conclusions

The present study was conducted primarily to determine the relative susceptibility of pleasant, unpleasant, and indifferent memory material to the retroactive inhibition resulting from the mastery of indifferent material during the interval before retention, when the affective material for a given subject is chosen on the basis of the subject's reactions. The learning material for each individual was a list of 15 words (learned by the anticipation method), chosen from a list of 100 on the basis of (1) the subject's judgment of each word as to its affective value, and (2) the subject's reaction to each word as registered by a Hathaway galvanometer. The retention of pleasant, unpleasant, and indifferent material under normal conditions for an interval of 48 hours is compared with the retention of such materials under the conditions of interpolated learning of indifferent material. The results justify the following conclusions:

1. The difficulty encountered in learning varies with the affective nature of the material being learned. The difficulty of learning increases as the affective tone of the material varies from pleasantness, through neutrality, to unpleasantness.
2. The efficiency of retention under normal conditions for an interval of 48 hours varies with the affective character of the material. Pleasant and indifferent material are retained much more effectively than is unpleasant material. Whether indifferent or pleasant material ranks first in the efficiency of retention is a function of the method of measurement employed.
3. The ability to retain pleasant, unpleasant, and indifferent material is detrimentally affected by learning indifferent material during the interval (48 hours) between original learning and retention.
4. The amount of retroactive inhibition, resulting from the interpolated indifferent material, is to some extent a function of the affective tone of the originally learned material.

The greatest amount of retroactive inhibition occurred for unpleasant material when comparisons are made in terms of the percentage saved for the time interval. According to relearning and recall scores, the greatest interference effect occurred for indifferent material. Pleasant material showed the least amount of retroactive inhibition according to all criteria of measurement employed.

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LA SUSCEPTIBILITÉ RELATIVE DU MATÉRIEL AGRÉABLE, DÉSAGRÉABLE, ET INDIFFÉRENT À L'INHIBITION RÉTROACTIVE

(Résumé)

On fait une comparaison de la rétention du matériel agréable, désagréable et indifférent dans des conditions normales pendant un intervalle de 48 heures et la rétention de ce matériel dans les conditions de l'apprentissage interpolé de matériel indifférent. On a divisé 125 sujets en six groupes; deux groupes pour chacune des trois sortes de matériel affectif employé. Les deux groupes pour chacune des trois sortes de matériel ont différé à l'égard de la manière dont l'intervalle a été rempli: aucun problème interpolé dans un cas et du matériel indifférent interpolé dans l'autre. Le matériel à apprendre par chaque sujet a été une liste de 15 mots, choisis dans une liste de 100 mots sur la base (1) du jugement de chaque mot par le sujet quant à sa valeur affective, et (2) la réaction du sujet à chaque mot, enregistrée par un galvanomètre.

On a trouvé les résultats suivants:

1. La difficulté de l'apprentissage s'accroît comme le tonus affectif du matériel varie d'agréable à désagréable, passant par l'indifférence.

2. Le matériel agréable et le matériel indifférent sont retenus beaucoup plus efficacement que le matériel désagréable dans les conditions d'aucun apprentissage interpolé.

3. Le matériel agréable a montré la plus petite quantité de rétroaction selon tous les critères de mesure; le matériel indifférent en a montré la plus grande quantité en termes des résultats du réapprentissage et du

rappel, tandis que la plus grande quantité de rétroaction s'est montrée pour le matériel désagréable en termes du pourcentage économisé pour l'intervalle.

BUNCH ET WIENTGE

DIE RELATIVE BEEINFLUSSBARKEIT ANGENEHMEN, UNANGENEHMAN, UND GLEICHGÜLTIGEN STOFFES DURCH RÜCKWIRKENDE HEMMUNG

(Referat)

Das Gedächtnis [retention] für angenehmen, unangenehmen, und gleichgültigen Stoff unter normalen Verhältnissen während 48 Stunden wird mit dem Gedächtnis für solchen Stoff unter den Verhältnissen der interpolierten Bemeisterung eines indifferenten Stoffes verglichen. Es wurden 125 Versuchspersonen in sechs Gruppen getrennt,—je zwei Gruppen für jede der drei verwendeten affektiven Stoffarten. Die zwei Gruppen für jede der drei Stoffarten unterschieden sich in Bezug auf die Weise, auf die die Zwischenzeit ausgefüllt wurde, von einander; in einem Falle wurde keine Aufgabe interpoliert und in dem anderen Falle ein gleichgültiger Stoff. Das Lernmaterial bestand für jede Vp. aus einer Liste von 15 Wörtern, die aus einer Liste von 100 Wörtern von zwei Ansichtspunkten aus gewählt worden war: (1) das Urteil der Versuchsperson über jedes Wort in Bezug auf dessen affektiven Wert; und (2) die Reaktion der Vp. auf jedes Wort, in der Registrierung durch eine Galvanometer erwiesen.

Man gelang zu folgenden Resultaten:

1. Die Schwierigkeit des Lernens nimmt mit dem Wechsel der affektiven Färbung des Stoffes von Angenehmheit durch Gleichgültigkeit in Unangenehmheit hinüber zu.
2. Angenehmer und gleichgültiger Stoff wird viel besser im Gedächtnis behalten, als unangenehmer Stoff, unter Verhältnissen, in denen kein Lernen interpoliert wird.
3. Bei dem angenehmen Stoff zeigte sich Hemmung, allen Kriterien der Messung nach, am mindesten; gleichgültiger Stoff lieferte, den beim Wiederlernen und Erinnern erzielten Zahlen nach beurteilt, den grössten Betrag an Hemmung, während unangenehmer Stoff dem für die Zwischenzeit ersparten Prozent nach [in terms of the percentage saved for the interval] beurteilt, den höchsten Betrag an Rückwirkung lieferte.

BUNCH UND WIENTGE

A TEST TO EVALUATE SOME PERSONALITY TRAITS*

From the Psychobiological Laboratory, Henry Phipps Psychiatric Clinic, The John Hopkins Hospital

Max F. Hausmann

Under the direction of Kurt Lewin, Ferdinand Hoppe carried out in 1931, at the University of Berlin, a series of experiments on "Success and Failure" (1). The results showed that the psychobiological experience of "success" and "failure" entails certain reactions which are characteristically different from one individual to the next.

An abnormal reaction to success and failure seemed to us of prime importance in the personality make-up of many of the patients who sought advice and treatment in a psychiatric clinic. In fact, it seemed that in many psychopathic cases an abnormal reaction to success and failure would have to be included in the very definition of their personality make-up. In order, therefore, to obtain comparable information on this point, we devised a *performance* which was to give us the *dynamics* of *success and failure* in as simple a form as possible. If such a situation is to be suitable, it must fulfill a number of conditions. In order to produce at all experiences of success and failure the performance has to be taken seriously by the subject; yet, it must not stir up the subjects too deeply as otherwise it could not be used on psychiatric patients; the instructions of the test must be simple, so as not to discriminate unduly between patients of higher and lower intellectual endowments; it must be such as to insure the best cooperation on the part of the patient; it must be inexpensive in capital investment and operation.

We adopted the following *procedure* which fulfills fairly well all the above-mentioned conditions: The subject is given 10 darts which he throws at a target. The target shows 6 circles; the values of the spaces between the circles are from 1-6, counting from the periphery to the center. After the subject has thrown the ten darts, the score is taken and written down. The subject throws another series

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of ten; again the score is taken and written down. This is repeated four or five times until the subject has a fair idea of his approximate level of performance.

At this point the actual test begins: The subject is told that from now on the scores will be reckoned in a different way; he has to make a bid for his score; if he actually throws the score for which he bid he is credited with that number of points; if he throws more, he is given credit for his contract only; no "bonus" is given for any points made above his contract. On the other hand, if the subject fails to make his contract he is penalized rather heavily; for each point he falls below the contract two points are subtracted from his bid. This is an example of how the rules work: Let us assume that a subject bids 34; now, if he actually throws 34, he is credited with this amount; if he makes more than that, e.g., 37, 40, etc., he is only given credit for 34; on the other hand, if he fails to make his bid, e.g., if he only makes 30, he falls 4 short; this is doubled and he is only given credit for 34 minus 8 = 26. Psychobiologically speaking, we have an experience of "success" in the case when the contract is fulfilled; an experience of "failure" when the contract is not fulfilled. The experience of "failure" may range from "very-near-success" to "very-far-from-success." The question arises to what extent it is possible to draw general conclusions from the reaction to success and failure in this sphere of motor performances. It seems to us that this depends on the degree to which a "score" of this sort is taken seriously by the subject. We found in almost every case that our subjects take the test quite seriously enough; as we mentioned before, we cannot use a test that would stir up the patients too much, as we would have to expect all sorts of untoward effects. The fact that once in a great while a subject fails to take the test seriously does not argue against the validity of the test, but it rather sheds light on the make-up of the person in question. We should not forget, however, that our relationship between the subject and the experimenter is of a particular nature and it is by no means certain that in dynamically different situations the results of the experiment would be equally satisfactory.

After the first bid is made, the ten darts are thrown again, the score is added up and the numerical result, and, therefore, success or failure, is announced to the subject. The subject, incidentally, is always allowed to look at his record, so that at all times he may obtain an accurate idea of what his performance has been up to that point. In the light of this result, the new bid is made; the darts are thrown again, etc. The total number of series thrown is 30, so that we have 25-26 single reactions to success or failure. It need not be pointed out that it is advantageous to have such a large number of single reactions as it allows us to obtain a fairly representative picture of the subject's action tendencies in this respect.

It is important that the subject should not know the real purpose of the test and that he take it as a "game," as "exercise," and a "test to measure his hand-eye coordination." We had no difficulty in convincing the patients of this.

In the reactions to the successes and failures a number of personality traits are brought into the open which it is otherwise difficult to observe in a sufficiently unequivocal situation. On the other hand, it is probable that any one of the character traits brought out in our performance can be observed in the behavior on the ward if the observation is continued for a sufficiently long time and if it is done by a trained observer. The advantage of a test like ours is that it brings the person into a situation where the traits can be observed within a short time and where the reactions find an overt expression which can readily be recorded in objective and quantitative terms, thus circumventing in the record the subjective interpretations of the observer.

The *results of the test* are recorded in the following manner: On the ruled paper the ordinate represents the scores, while the abscissae represent the number of the series.

The score "bid" is indicated thus: O; the score made thus: . (a dot) In case the contract is fulfilled the two points coincide; if the contract is not fulfilled . (a dot) is below O; the distance between the two points indicates the degree of "failure." If the actual score thrown is higher than the contract, an X indicates the actual score.

The curves may be analyzed with regard to the following points: The subject may be quick or slow to adjust his bid upward; he may be quick or slow to adjust his bid downward; his adjustments may be made by abrupt, large steps or by conservative, small steps; the adjustments may be "irrational"; i.e., the individual raises his bid after a failure and lowers it after a success; he may be "sensitive to load" in the sense that he is well able to throw a certain score, but as soon as he raises his bid a little, his score falls off very sharply; there are individuals who have "resistance levels" below which they are very reluctant to go, and subjects who lack such a level.

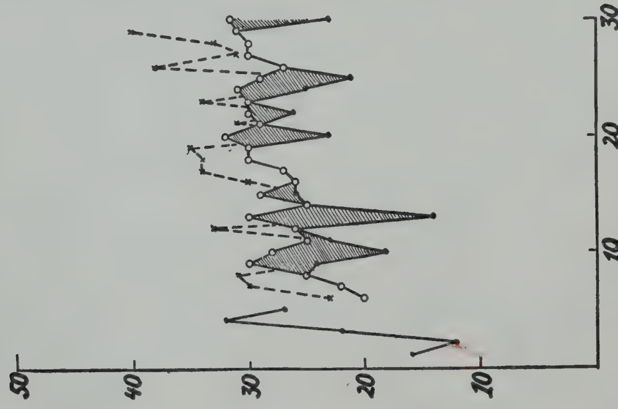

FIGURE 1

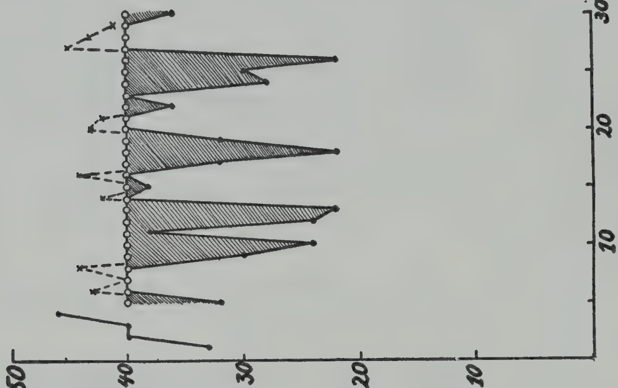

FIGURE 2

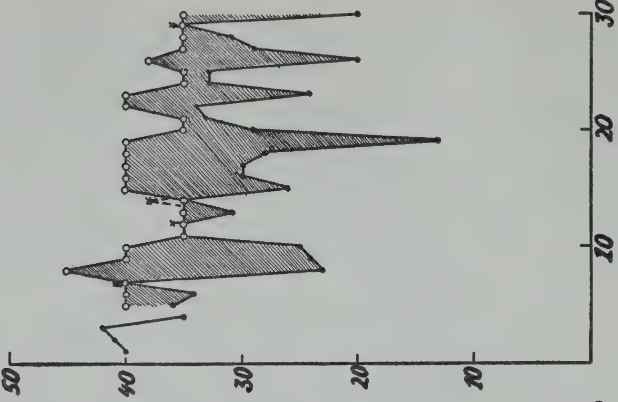

FIGURE 3

Figures 1-6 will show more clearly what we have in mind with these terms:

In Figure 1 we find that during the five trial series the scores were 16, 12, 22, 32, and 18. On this basis the patient bids 20, throws 23 and makes her contract; raises her bid to 22, throws 30 and makes her contract; raises again reasonably to 25, throws 31 and makes her contract; now, getting bold, the patient jumps to 30, fails and makes 24; at once she lowers the bid to 28, fails and makes 18; a further adjustment brings her bid to 25, which she nearly makes (23); without having made her previous bid she raises it now to 26, makes 33 and makes her contract; now advances boldly to 30, fails and makes 14; at once she recedes to 25 and barely makes her contract; thereupon she advances to 29, fails to fulfill her bid and makes 25; lowers her bid to 25, throws 30 and makes her contract; advances cautiously one step to 26, throws 33 and makes her bid; now advances more boldly to 30, throws 33 and fulfills the contract; she maintains her bid at 30, throws 35 and makes her bid; now raises the bid to 32, fails and makes 22; immediately lowers her bid to 29, throws 31 and makes her bid; again raises her bid to 30, fails, and makes 26; now for the first time after a failure, she maintains the contract at the same level, throws 34, and fulfills the contract; now advances the contract again to 31, fails and makes 25; she at once lowers her bid to 28, still fails (23); and once more adjusts downward to 26, throws 38 and makes the bid; raises again to 30, makes her contract (31); maintains the bid at 30, makes it again (33); now she advances her bid to 31, and makes it (40); and as the last contract (known to her as such) she advances her bid to 32, but fails and makes 23.

In summary, this individual presents adjustments of her estimate which are always entirely determined by the last result; the previous results or the "long-term trend" are not considered at all. Without any lag or resistance, the bids follow success or failure very closely.

These are the facts upon which the subjective interpretation of the performance can be based. We feel that an individual who shows a similar behavior is of an unsteady make-up, easily discouraged, unstable, whose self-confidence cannot withstand undamaged any pressure from environmental forces. An interesting sidelight is shown in the relation between the instability shown on the psychobiological level and the form of her blood sugar tolerance curve, which indicates a certain instability of the autonomic nervous system.

Figure 2 shows an entirely different picture. Here the patient decides upon a certain, rather high contract and clings to it, no matter what happens. The bid is from the start fixed at 40 and never varies whether she loses or whether she wins. This rigidity and failure to comply with the realities I would like to interpret as "stubbornness," "perseverance pushed to an absurd, pathological degree." Interestingly enough, paranoid patients have presented this type of performance, thereby betraying the rigidity of the make-up. The results of the test can here be used therapeutically to advantage. The physician has the record of the actual performance by means of which he can show to the patient how much better he had fared if he had proved himself more willing to adjust to the realities.

The next, Figure 3, reflects again quite a different personality. Here the performance can be described thus: The preliminary trials have shown to the patient that his approximate level of performance is around 40. His first bid is 40, he fails to fulfill his bid and makes 36; but he maintains this bid (40), again fails and makes 34; he maintains his bid and this time succeeds, but with no margin at all. Nevertheless, he raises his bid at once to 45, fails and makes only 23. Now he lowers his bid to 40, fails and makes 24; he maintains his bid at 40, fails and makes 26. Now the bid is lowered to 35, which is made. For the following three bids this bid is maintained, twice successfully and once with a small failure. Thereupon the bid is advanced to 40, he fails and makes 36; he maintains the bid at 40, fails and makes 30; he maintains the contract, fails and makes 30 again; the bid is further maintained at 40, he fails and makes 28; the next bid is again 40, but the score drops to 14. Now he adjusts his bid to 35, fails and makes 28; he maintains the bid, fails, but comes quite close to making it (33). Now the bid is advanced once more to 40, he fails and makes 34; the bid is maintained at 40, but again he fails and makes 24. Thereupon, the bid is lowered once more to 35; on two occasions he fails, but comes close to making it (33). Nevertheless the bid is now raised to 38, the score drops to 20; and an adjustment is made to 35. This bid is maintained until the end with success once and failure in three cases.

This record then shows a raising of the bid to an unreasonably high level on the slightest provocation. The reluctance to adjust the bid downward is almost equally marked. We may interpret such a performance as the expression of a markedly aggressive individual,

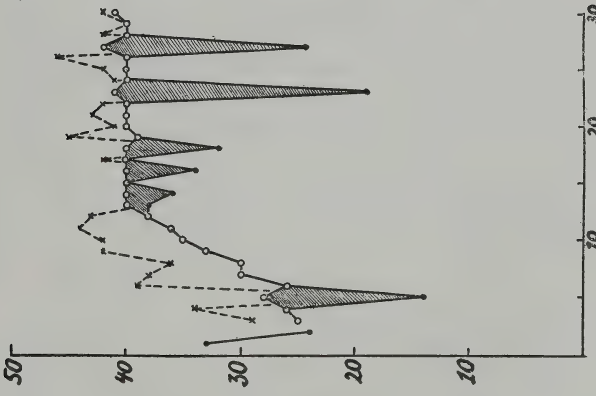

FIGURE 4

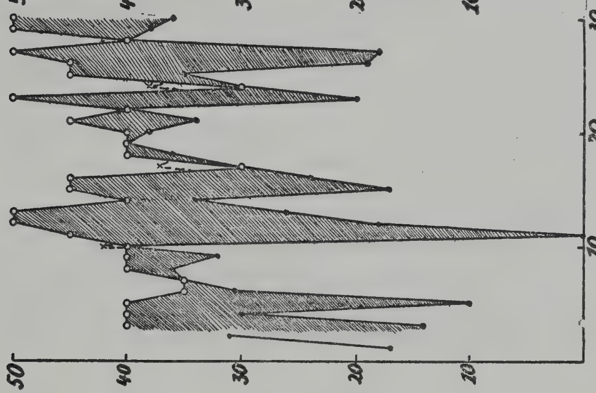

FIGURE 5

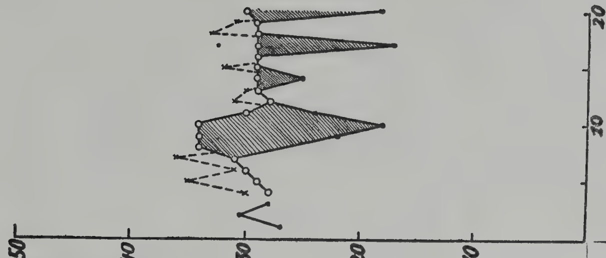

FIGURE 6

whose ambitions are not successfully controlled by and related to the actual capacities.

Figure 4 is interesting for other reasons than any of the previous ones. The performance is to be described as follows: After two trials a first bid of 25 is made. This contract is fulfilled and the subject moves one point up to 26. This contract is also made with a fairly large margin. Now the contract is moved up two points to 28. Verbal statements indicate that the patient is greatly concerned over her own boldness. In fact, now she fails to make the bid and drops off to 14. An immediate adjustment to the previous level brings success by a very wide margin. During the following six series the bid is gradually increased and regularly made by wide margins until the contract of 40 is reached which somehow seems to frighten the patient. For the next six series the bid is maintained bravely in spite of variable success. Then the patient becomes emotionally adjusted to the new level of expectation and for four series the contract is made with good margins. When, however, the bid is raised one point (with verbal expressions of apprehension and misgivings), the score falls very low to 18. An immediate adjustment brings again success for three successive trials. Again the bid is cautiously raised and again the score falls far below the contract (24). Again retracing her steps the patient successfully throws the next two series and finally at the last trial raises her bid successfully to 41.

We have here a patient who has a fair idea of her capacity and also shows fair steps of adjustment. Her great weakness lies in her "sensitivity to load"; i.e., as soon as the responsibility placed upon her increases, she becomes so frightened and upset that her performance shows a very marked falling-off. We deal with an individual whose sensitivity to external or internal forces is much greater than in the average individual; this sensitivity finds an objective expression in a marked falling-off of the performance under any sort of strain.

Figure 5 represents another type of behavior in this situation. Even without any close inspection of the sequence of the adjustments we see at once utterly erratic reaction. "Irrational adjustments" are made: the bid is raised after a failure and lowered after a success, abnormally large steps of adjustment are found. Nevertheless, all through the test a certain aggressiveness shows which at a later stage, when the patient had improved somewhat, became more pronounced.

We feel justified here in speaking of a definite impairment of judgment. The patient showed a schizophrenic reaction.

The last figure (Figure 6) represents the following picture: During the three trial series the approximate level was established around 28. The first bid made is 28, the subject makes 30 and fulfills his contract with a margin of 2 points. He raises his bid to 29 and makes his bid with a margin of 6; again his bid is raised slightly to 30, then to 31, then to 34; always successfully. The bid of 34 is maintained during the following two series in spite of rather heavy losses. The realization that the bid is too high brings about an adjustment to 30, and when this also proves unsatisfactory, to 28. This time the contract is made and the bid is advanced to 29. In spite of occasional losses this level is maintained until the last series, when a raise to 30 is made, which happens to be unsuccessful. We have here reasonable adjustments up and down in small steps; a certain resistance to losses which does not become stubbornness and is properly termed "perseverance." This subject is a graduate student at the University.

In summary, then, we may say that we have presented here six typical cases (out of a total of about 150 records) of the behavior of various individuals in a new test situation. This test situation is so arranged as to give the subjects experiences of success and failure of moderate intensity. We found variations in the reaction which differ greatly from one individual to the next and which can fairly be assumed to be representative of the individual's behavior in similar situations. The test is so simple as to allow even subjects of low intellectual endowment to understand the game. The cooperation is almost always very good; the subject takes the test more or less as he would a game; the cost of the equipment and the operating expenses are very low.

While most of our records have been obtained in working with patients, an attempt is now being made to use the method for an investigation of the psychobiological reactions to success and failure of "normal" adolescents and children.

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UN TEST POUR ÉVALUER QUELQUES TRAITS DE PERSONNALITÉ

(Résumé)

Une réaction anormale au succès et à l'insuccès semble caractéristique de plusieurs personnes qui cherchent des conseils ou du traitement à une clinique psychiatrique. Pour obtenir des données objectives et comparables sur ce trait de personnalité, on a inventé une expérience pour nous permettre de voir en action la dynamique du succès et celle de l'insuccès dans une forme aussi simple que possible.

On a adopté le procédé suivant: on donne dix dards au sujet lesquels il jette à une cible; après cela on note les résultats et les écrit. Après quatre ou cinq fois le sujet sait assez bien le niveau approximé de son exécution.

Puis on commence le vrai test: on demande au sujet de dire ce qu'il va accomplir. On arrange le système de sorte que le sujet désire prédire des résultats aussi élevés que possible sans s'exposer à trop de risques (de grandes peines s'il ne fait pas ce qu'il prédit). Il jette les dards encore une fois et on lui annonce les résultats—le succès ou l'insuccès. Sa réaction est montrée par sa prédiction suivante qui peut être ajustée ou non ajustée à la tendance selon les différences individuelles.

On consacre le reste de l'article à une analyse de six personnes de traits différents. La Figure I montre une courbe qui montres des adjustements extrêmement instables et un manque de persévérance. En contraste avec ce malade-ci qui se décourage facilement et se laisse influencer par toutes sortes d'impulsions, la Figure 2 est le tableau du rendement d'une malade qui est très rigide et entêtée dans ses réactions. Cette rigidité expliquera un peu les tendances paranoïdes de cette malade.

La Figure 3 est le rendement d'un individu aggressif dont les ambitions ne sont pas gouvernées par les capacités qui existent. Dans ce cas comme dans tous les autres il se trouve une corrélation très satisfaisante entre les résultats du test et le comportement dans la vie quotidienne. La Figure 4 est un bon exemple d'une personne qui est très sensible à des responsabilités qu'on lui donne; elle montre une "sensibilité pour la charge," c'est-à-dire, un certain affaiblissement de son exécution quand elle est "assez audacieuse" pour prédire une meilleure exécution. La Figure 5 est l'exécution variable d'une manière surprenante d'une personne schizophrénique de bonne heure dont le comportement superficiel a été assez bon à l'époque du test. La Figure 6 représente enfin la persévérance moyenne et les ajustements sensés d'un adulte intelligent normal.

Hausmann

EIN TEST ZUR BEWERTUNG EINIGER PERSÖNLICHKEITSEIGENSCHAFTEN

(Referat)

Eine abnorme Reaktion auf Erfolg und Miserfolg scheint für viele der in einer psychiatrischen Klinik Rat und Behandlung suchenden Personen karakteristisch zu sein. Um über diese Persönlichkeitseigenschaft objektive und vergleichbare Befunde zu erhalten, wurde eine Tätigkeit erfunden, die es uns ermöglichen sollte, in einer möglichst einfachen Form die Dynamik des Erfolgs und des Miserfolgs aktiv zu beobachten.

Es wurde folgendes Verfahren verwendet. Es werden der Versuchs-

person zehn Pfeile gegeben, die sie einer Zielscheibe entgegenschiesst. Danach wird die erzielte Zahl notiert und niedergeschrieben. Nach 4 oder 5 Versuchen kennt die Versuchsperson ziemlich genau das approximative Niveau ihrer Leistung.

Danach fängt die eigentliche Prüfung an. Es wird der Vp. gesagt, dass sie auf ihre Zahl zu bieten [bid] hat. Das Bietungsystem ist so eingerichtet, dass es in der Interesse der Vp. liegt, so hoch zu bieten wie sie kann ohne übermässig viel zu riskieren. (Es gibt hohe Strafen bei Nichterfüllung des Kontraktes). Die Pfeile werden wieder geschossen und das Resultat (Erfolg oder Miserfolg) wird der Vp. gemeldet. Ihre Reaktion hierauf wird in der folgenden Bietung erwiesen, welche der Richtung der Leistung, den individuellen Unterschieden nach, vielleicht, oder vielleicht auch nicht, angepasst sein wird.

Im Übrigen beschäftigt sich die Abhandlung mit einer Analyse sechs karakteristisch verschiedener Personen. In der Abbildung I wird eine Kurve gegeben, in der extrem labile Anpassungen und Mangel an Beharrlichkeit erwiesen werden. Ein starker Kontrast zu diesem Patient, der leicht entmutigt wird und von allerlei Antrieben gelengt wird, ist in der Abbildung 2 dargestellt, welche die Leistung einer Patientin schildert, die in ihren Reaktionen sehr steif und hartnäckig ist. Diese Hartnäckigkeit erklärt wahrscheinlich teilweise die paranoiden Neigungen dieser Patientin.

Abbildung 3 zeigt die Leistung eines aggressiven Individuums, dessen Ehrgeiz von seinen eigentlichen Fähigkeiten nicht regiert wird. In diesem Fall wie in allen übrigen Fällen gab es eine sehr zufriedenstellende Korrelation zwischen der Testleistung und der alltäglichen Benehmensweise. Die Abbildung 4 zeigt uns ein klares Beispiel der Leistung einer weiblichen Person, die für die ihr aufgelegten Verpflichtungen sehr empfindlicht ist. Sie erweist "Empfindlichkeit der Aufbürdung gegenüber" ["Sensitivity to load"]; d.h., die Leistung nimmt an Tüchtigkeit ab, wenn sie "dreist" genug gewesen ist, die Bietung zu erhöhen. Abbildung 5 zeigt die erstaunend umherirrende Leistung eines anfangenden Schizophreniefalles, dessen oberflächliches Benehmen zur Zeit der Prüfung ziemlich gut war. Abbildung 6 stellt schliesslich die mittelmässige Beharrlichkeit und die verständigen Anpassungen eines intelligenten, normalen Erwachsenen dar.

HAUSMANN

ON THE OBJECTIVE PROOF OF THE EXPERIMENTAL PERSONALITY AGE REGRESSION*

From the State Psychoneurological Institute of Ukrainia

K. I. Platonow

One of the most interesting phenomena which can be brought about in the hypnotic state is the personality transformation in the form of a regression of an adult to childhood as the result of a verbal suggestion. Although this phenomenon has been known for a long time, there are many who consider this effect of the influence of speech as highly striking, and some go so far as to deny it completely.

Our present state of knowledge regarding the higher activities of the nervous system enables us to explain satisfactorily the mechanisms of physical activities as well as the phenomenon of suggestion in a waking state and in the state of experimental sleep, or hypnosis. Nevertheless, some of these phenomena artificially evoked by means of a verbal suggestion are attracting a great deal of attention and need a more objective, scientific explanation. The personality age regression is one of this group of phenomena.

Those acquainted with experimental hypnotism and the literature of the field are aware of the fact that an adult in the hypnotic state on receiving the verbal suggestion that he is six years old will, after awakening, behave according to the suggested age, showing exactly those traits characteristic of the age. This transformation bears as well on the scope and quality of ideas, emotions, and attitude toward the surroundings as on the corresponding variations of speech, handwriting, and other external motor reactions.

The observation of this kind of experiment has always provoked the question as to whether the phenomenon is a fact or an artifact. Is it a real reproduction of childhood or an expression of imitative art, a talented acting of "hystericals" who are simply misleading the naïve investigators? Dr. Moll in his introduction to the second German edition (1926) of the research on this problem carried out

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in 1893 by the renowned psychiatrist, Kraft-Ebing, emphasized the point that the question had not received a definite solution up to that time. The fact that in Germany it was considered necessary to undertake a new edition of the investigations of the last century is evidence of the growing interest in this phenomenon.

The above investigations, although offering no scientific interpretation, do supply some proof, scientific enough for the epoch, that this phenomenon is a fact and not an artifact. This evidence has, however, met with highly adverse criticism on the part of authoritative circles despite the fact that the argumentation, with sufficient scientific basis for the period, was advanced by so learned a clinician and so scrupulous an analyst as Kraft-Ebing. For the first time, in 1892, Kraft-Ebing attempted a scientific investigation, which remained unique until the present time, of the personality regression to childhood of Miss K. Pieget, 33 years of age, an individual who was healthy and by no means hysterical, and who had a good hereditary background. An experimental demonstration at a meeting of neuropathologists and psychiatrists in Vienna in 1893 provoked some doubt as to the genuineness of the suggested states. Professor Benedikt of Vienna went so far as to call this experiment a "silly humbug." In his opinion Kraft-Ebing was a victim of an imposture by a hysterical woman. "For a psychologist," says Professor Benedikt, "it would be very easy to disclose this comedy." It is a regrettable fact that, as far as the literature shows, no psychologist has yet been able to disclose this "comedy," probably because of the inadequacy of the subjective psychology which dominated Western Europe to present such evidence. The task remained for the "objective" psychology, the reflexology of Bekhterev, to throw light on the problem.

Did Kraft-Ebing give sufficient proof in support of the genuineness of the personality transformation, and what were his arguments? When first starting his research on suggested age states, he sought to throw light on the question of whether such cases were real reproductions of the earlier personality reinstated from the subconscious by some mechanism, or whether they were simply acting. On the basis of his earlier observations he was inclined to consider them a reproduction of a real and not phantastic personality, and undertook to prove it experimentally. In his opinion it could be done by the following methods: (1) frequent repetition of the experiment with a comparsion of the personalities called forth at each;

(2) the evaluation of these states by those who knew the subject in childhood; and (3) comparison of the handwriting of the subject while in the state of age regression with the original handwriting of his childhood.

These methods, with the exception of the last, cannot be considered strictly objective or scientific. The first and, particularly, the second methods involve too much of subjectivism. Further the second method is not always easy to carry out. Kraft-Ebing took advantage of a favorable opportunity to compare an available sample of early handwriting with the experimentally produced handwriting and was able to perform the experiment in the presence of the mother of one of the subjects who reported the complete identity of the age states. On the basis of these experiments the author came to positive conclusions, considering these phenomena a real revival of the subject's earlier life.

Despite the favorable experimental conditions, the conclusions drawn by Kraft-Ebing have by no means met with general agreement. While the majority of hypnotists adhered to his standpoint, some, up to the present, prefer to agree with the opinion of Professor Benedikt, quoted above. Kraft-Ebing himself states that there are no substantial psychological proofs available. It is evident that the previous level of knowledge in psychology as well as in physiology was not sufficient to allow adequate investigations and explanations of such phenomena. Furthermore, such experiments were believed to be harmful to the psychological integrity of the personality, the subjective psychologist holding that such personality splitting could have but a detrimental effect on the subject. With the present level of knowledge in psychology and with our knowledge of the physiological functions of an animal organism, these apprehensions can be regarded as devoid of any foundation.

Since the beginning of the last decade of the nineteenth century, the period of the development of experimental psychology, the methods of this branch of science have been much expanded and improved. Furthermore, the Sechenov-Pavlov-Bekhterev doctrine, based on physiological experimentation, has given an interpretation to phenomena whose mechanisms could not be elucidated by subjective psychology. If positive results should be found in the investigation of age regression, they would, in the author's opinion, be of outstanding importance in confirming that principle of experimental psychology which states that each experience leaves its trace in the memory and can be revived under suitable conditions. The value of hypnosis in experimental psychology would also be emphasized.

Hence, the application of new scientific methods to the study of this interesting problem in an attempt to elucidate the phenomenon seemed expedient. The present state of the different branches of sciences studying the integrated personality provides much more favorable conditions for this research than formerly. The methods of experimental psychology including measurements and statistical treatment are characterized by a larger outlook and the highest degree of objectivity. The method we considered most suitable for our purpose was the Binet-Simon testing method. By means of definite coefficients the qualities of grades of perceptions and reactions at different periods of the individual's life can be differentiated. Allowing a more objective evaluation, it gave us the possibility of a more accurate qualitative and quantitative estimation of ideas and reactions at different age levels which was quite indispensable for our purpose, and which could supply the necessary objective proofs. This method was applied in all our experiments.

The investigations were performed with two women and one man, all high-school graduates. We carefully avoided choosing for this purpose so called "hystericals" with definite clinical signs of hysteria. No one of our subjects belonged to this category.

Subject No. 1, N. D., a housewife, age 39, in spite of the hard conditions of her life, has shown neither "hysterical" reactions nor lost her neuropsychical balance. Nervous system showed no signs of pathological changes. Pulse rate, 63-66; Aschner, 66-60; clinostatical pulse rate, 4 beats slow. Her plethysmograms and pneumograms showed a rather sensitive and ready reactive mobility. She yielded easily to hypnotic sleep and exhibited a posthypnotic amnesia.

Subject No. 2, J., a school mistress, age 34, was also a well-balanced subject and an efficient worker without any clinical symptoms of "hysteria." Aschner, 72-64; clinostatical, 78-72. Yielded readily to hypnotic sleep.

Subject No. 3, A., a student, age 20. According to his own statement suffers from a disturbance of the autonomic nervous system in the form of attacks of asthma. Neither central nor autonomic nervous systems showed any objective pathological symptoms. Yields readily to hypnotic sleep.

All of these subjects showed from the first experiment a quick development of the conditioned reflex inhibition during hypnotic sleep, and a vivid and ready reaction to verbal stimuli of most varied content, and thus belong to the category of the so-called "somnambulists." The results were not quite uniform regarding the character of their motor reactions. No one of them manifested a definite mobility with vivid motor reactions. Externally they showed a great reserve. Subject J., in particular, gave the impression of a well-balanced, matter-of-fact personality. Subject N. D. proved to be more lively and more motile in her motor reactions. These individual traits of the subjects were reflected in their behavior during the suggested age regression during the investigations as well as when they were observed while alone.

The subjects were not, therefore, carriers of a hysterical neurosis. Nor were they "hystericals" or "grand sujets" of the Charcot type, who alone were supposed to possess the faculty producing such hypnotic phenomena. Neither their susceptibility to hypnosis nor their somnambulism can be regarded as criteria of hysteria according to the modern concepts of hypnosis and the organization of the central and autonomic nervous systems. The new biological conception of hysteria as an original individual defense reaction, on the one hand, and the present classification of the nervous system according to its types, which is the Pavlov-Bekhterev doctrine based on physiological experiments, are requiring a new interpretation of "somnambulism."

This physiological knowledge enables us to discriminate types of normal nervous constitutions which differ as to excitability, development, and the quality of responses. On the basis of experimentation performed by Professor E. Katkov and the author, which consisted of using the conditioned response method with a large number of somnambulists, it can be concluded that the latter can be classified among a biologically valuable category of normal individuals. Consequently, these results allow us to disregard completely the erroneous judgment which considers the corresponding nervous system responses as a manifestation of hysteria, according to the earlier concept, including "falsity," "slyness," "affectation," and the like, terms used to stigmatize those individuals showing pathological responses to a difficult, unbearable situation.

Our subjects thus belong to one of the types included within the normal classification, and cannot be considered hysterical in the usual trivial sense of the word. Subject A. cannot be regarded as possessing good health, but there were no data to justify his classification as a hysteric. Apparently Kraft-Ebing's subject also belonged in one of the categories of the newly outlined types within the normal organization of the nervous system.

THE EXPERIMENTS

It should be pointed out that none of the subjects was told the nature or aim of the experiments. Each subject was seated at a table, and, after some questions regarding his health, was hypnotized. The whole process of hypnotizing took no more than a few seconds, the subjects actually going to sleep after being told repeatedly to do so. We tried in all cases to bring the subject to a state of deep suggested sleep, the judgment of this being controlled by blood-pressure measurements before the experiment and during the hypnotic state. The results of the measurements supplied an objective criterion of the hypnotic state, since, according to the investigation of Dr. Tzinkin, one of our workers, the blood pressure is lowered during hypnotic states. The following data, obtained in the course of the experiment, strengthened his conclusions:

Subject	Blood pressure before experiment	Blood pressure during hypnotic state
N. D.	130	120
J.	118	106
A.	129	115

During the investigations all conditions required for strictly scientific experimentation were observed, and the subjects were hypnotized in the presence of physicians, students, and social workers. After the subject had reached a suitable state of hypnosis, we generally addressed him as follows:

"At present you are six years old." (This suggestion was repeated three times.) "After you wake up you will be a child of six. Wake up!"

After the subject was awake, a short conversation was held with him, for orientation purposes, and this was followed by tests according to the Binet-Simon method. By means of suggestion the subjects were transferred to the ages of four, six, and ten. When transferred from one age to another they were hypnotized, given the corresponding suggestion, and awakened again. The experiments were generally ended by the suggestion of the real age, and

TABLE 1

Age	Test				
15	Drawing conclusions	Interpreting pictures	Repeating 26 syllables	Rhyme finding	Repeating 7 digits
13	Restoring word order	Defining 3 abstract conceptions	Giving 60 words in 3 minutes	Sentence building	Resistance to suggestion
10	Sentence building	Answering abstract questions (complex)	Finding inconsistency in short story	Drawing 2 forms after removal of model	Arranging weights
9	Answering abstract questions (simple)	Giving months in order	Giving value of current money (when seen)	Defining 5 household objects	Giving change (money)
8	Repeating 5 digits	Giving correct date	Finding missing part in picture	Counting backwards (20-1)	Comparing 3 pairs of objects out of visual field

TABLE 1 (*continued*)

Age	Test				
7	Naming 5 basic colors	Giving sum of coins of different values	Simultaneous executions of commands	Tracing circle around each of 3 squares	Giving right and left side
6	Aesthetic comparison (3 pairs of faces)	Counting 13 coins	Drawing a rhomb	Description of household objects	Giving time of day
5	Counting 4 coins	Adjusting 2 parts of puzzle, given sample	Repeating 10 syllables	Copying square	Comparing weights
4	Comparing lines	Repeating 3 digits	Naming several common objects	Giving own sex	
3	Giving own name	Naming objects in picture	Repeating 2 digits	Repeating 6 syllables	Pointing to nose, mouth, eyes

were followed by amnesia. The subjects had no idea of the states just undergone, nor had they any knowledge of the experiment which was performed. It may be added that after the experiment they felt as strong and healthy as before.

Table 1 shows the tests corresponding to different age levels (according to the Binet-Simon method) which were used in our investigations. In both the table and the figures the age level is read on the vertical axis and the number of the test on the horizontal axis. Only summarized general results, illustrated by the figures, are given.

RESULTS

Subject N. D. Figure 1 shows the results of testing the subject during a regression to the age of six. The experiment was begun by giving her the 9-year tests in order to test her responses to more complex problems. The subject failed in all these tests, and passed two 8-year tests, four 7-year, three 6-year, and all the 5-year tests. The record of the experiment shows that the subject was essentially childish, notwithstanding the fact that she was actually an adult of normal intelligence. Summarizing the results, N. D. obtained a mental age normal for a child of six.

Figure 2 gives the results for the age of four. Here the subject passed all the tests for this age, three at the 5-year level, and failed all at the 6-year level. According to the experimental record, the adult N. D. gave answers corresponding to those of a four-year-old and behaved according to the same age. She was capricious, naughty, and fidgety. Mental age was normal for a child of four.

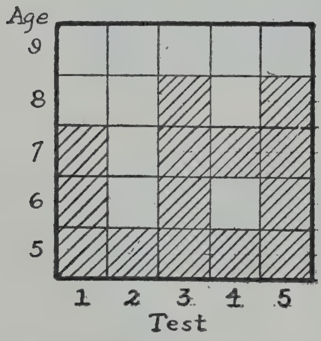

FIGURE 1

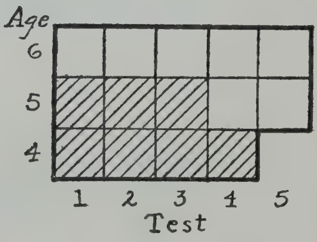

FIGURE 2

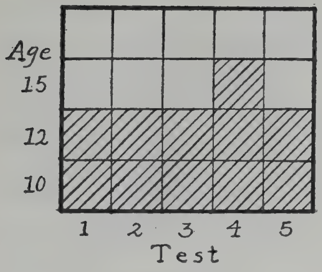

FIGURE 3

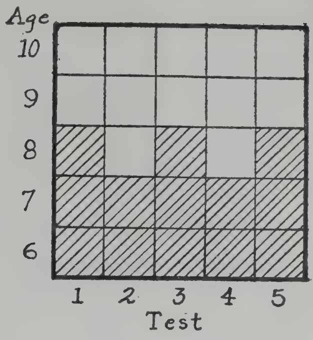

FIGURE 4

Figure 3 shows the data for the last experiment with N. D., at the age of ten. It can be seen that she passed all the tests for the ages of 10 and 12, one for 15, and none for the adult levels. The mental age is normal for a ten-year-old.

Subject J. Figure 4 presents data for the suggested age of six. J. passed all the 6- and 7-year tests, and failed all the 9- and 10-year tests. Although the "six-year-old" subject failed all the 9-year tests she was given the 10-year tests, a slight deviation from the Binet-Simon method, in order to observe her behavior in this situation. Mental age was normal for the age of six.

Figure 5 gives data from the investigation at the age of four. The subject passed all the 3-year tests, failed one of the 4- and all of the 5-year tests. The mental age is normal for this age.

Data for the ten-year-old J. is given in Figure 6. All the 10- and 12-year tests were passed, and one of the 15-year tests. The

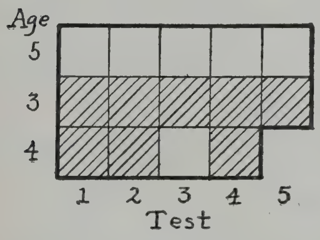

FIGURE 5

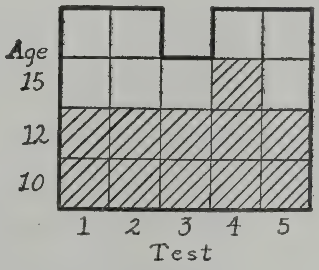

FIGURE 6

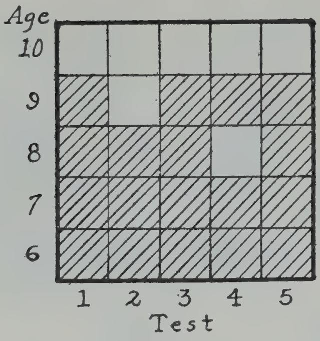

FIGURE 7

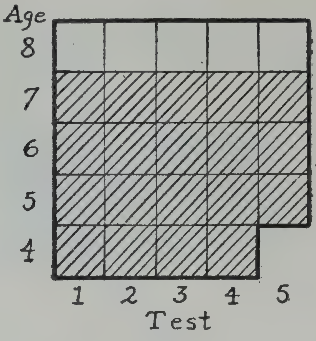

FIGURE 8

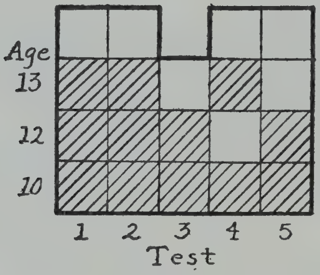

FIGURE 9

subject failed all the adult-level tests completely. Her performance was indicative of a mental age normal for the 10-year level.

Subject A. Figure 7 shows performance during the "age of six." A. performed all the 6- and 7-year tests, and all but one of the 8- and 9-year tests. None of the 10-year tests was passed. The mental age is normal.

Figure 8, showing results at the age of four, disclosed the fact that the subject had an intelligence beyond the normal. At this age he performed all the tests intended for the ages of 4, 5, 6, and 7 years, although he failed all the 8-year tests. Apparently, we had to deal with a "boy" of premature development.

At the age of ten, Figure 9, A. passed all the 10-year tests, four of the 12-, and three of the 13-year tests, and failed all the adult-level tests. Mental age is normal for ten.

As the above experiments show, each one of the subjects at a given suggested age was able to pass only those tests which did not go beyond the normal level established by the Binet-Simon method for corresponding ages. The intelligence estimation in our "children" according to the experimental data would be exactly the same as such estimation according to the experimental investigation of real children. On the basis of the experimental data all of our "under-age" subjects could be classified as normal children, with the exception of A., who at the age of four showed a premature development. It should be pointed out, however, that at the present time he is one of the brightest students of a technical school.

It was found, as indicated above, that the behavior of the subjects during the age regression reflected the suggested age in all its details. When four or six years old, for instance, they were capricious, fidgety, easily distracted, and fatigued. When given tests too complicated for that age they declared that they would not go on, etc. At the age of ten, separated from the previous age by several minutes only, their behavior changed completely. They worked quietly, attentively, and displayed a steady interest in the work. The handwriting of each of the subjects in the suggested age corresponded with the latter.

Would it be possible, taking into account the above data, to assume a simulation, a talented acting, or a mystification by "hystericals?" The last supposition cannot be regarded as valid in connection with the absence of clinical symptoms of hysteria in our subjects, as stated above. To consider it acting, we should have to assume, on the one hand, exceptional creative capacities, and, on the other hand, a strange coincidence in the selection of three personalities endowed with an outstanding creative gift. The observation of our subjects in their real life bears out neither one of these suppositions. Though one of the subjects, N. D., possessed to a certain degree a talent for acting and had been practicing this art previously as an amateur, this ability did not go beyond that of an average amateur actress. Our reiterated requests to N. D. in a waking state, as well as to the other subjects who were absolutely devoid of any talent for acting, to simulate a child of six or ten remained unsuccessful. They were declined with obvious bewilderment and embarrassment. When asked to perform the tests devised for these ages as they would be performed by children of four, six, or eight years of age, the subjects N. D. and A. fulfilled the task in a manner quite different from their own performances during the hypnotic state. At the same time, in the waking state, they easily passed all the tests including those in the adult levels.

In the course of our experiments we met with one more phenomenon which has an important bearing upon our conclusions, and which can be considered as a proof of a real personality reproduction. The process of investigation revealed the *unity* and *stability* of a definite complex of knowledge and responses to the same stimuli in the course of repeated suggestions of the same age. This same fact holds true for the handwriting of the subjects; repeated investigations of the same suggested age showed the identity of caligraphical as well as orthographical character of writing, although in their normal ages the subjects failed to imitate the handwriting of a child.

All the above data and considerations allow us to exclude the assumption of any kind of simulation. A special proof against this assumption is supplied by the mental age obtained by the Binet-Simon method, and which was found to be normal for the different age levels, and therefore allows us to assume a real reproduction of the remote periods of the subject's life. Apparently, the Binet-Simon method does not bear out Professor Benedikt's statement as quoted above.

The empirical subjective psychology of the past with its abstract conceptions has failed to give an interpretation of the mechanisms of "psychical activities." Today, when personality investigations are performed on the basis of animal study, such phenomena as those in question can be attacked and interpreted from another standpoint, and under more favorable conditions. The reproduction of the traces of the remote life impregnated in the personality can be proved not only by the methods of experimental psychology but from the purely physiological viewpoint also.

Kraft-Ebing's statement that the phenomenon of suggested age was too complex for a psychological interpretation was justified until the present time. These difficulties may now be avoided as a result of the new trend of scientific research in the field of mental processes, i.e., human behavior. These investigations are built entirely on the basis of the natural sciences and tend to penetrate the physiological rather than the psychological field. Loeb's doctrine of tropisms, Pavlov's "true physiology of the brain," Bekhterev's reflexology with its anatomically and physiologically based energy doctrine, and, finally, the American behaviorism, enable us to build up a new concept of personality, to devise new methods of investigation, and to give a new interpretaton of the structure as well as of the dynamics of personality.

Subjective psychology, as we have said, failed to supply Kraft-Ebing with a satisfactory explanation of the results of his experiments. He started with a question which always presented the greatest difficulties: "How can we explain the fact that a single word can call forth a whole series of reminiscenses?" This question, according to Kraft-Ebing, was the most difficult that psychological intepretation had to meet. Of what help has subjective psychology been to Kraft-Ebing? It only enabled him to cite, as a kind of proof, the idea Lessing expressed in his "Nathan the Wise": "How many of such pictures profoundly impregnated in ourselves are allowed to form before the moment when they are called into life by a single word, a unique sound!" He could give no more scientific psychological proof than this. Nor did the subjective psychology enable Dr. Moll to give a scientific interpretation of the problem of how the single word "wake-up" can change entirely the whole condition of the subject. This point is the foundation of his skepticism toward all the previous theories of the physiologists concerning hypnosis and the phenomena evoked during the hypnotic state.

Our modern conception of the higher activities of the nervous system, Pavlov's "true physiology of the brain," does give an adequate, scientific interpretation of all these problems. At the present time we are well aware of the rôle of words as conditioned stimuli in the life of the organism. Without going into detail, we shall only point out that in the light of the conditioned reflex hypothesis, the word, far from being an indifferent stimulus for the animal organism, brings about in the latter a great variety of physiological reactions. The word continually associated during the individual's life with the basic stimuli evoking organic reactions is itself able to bring about the same changes, and eventually very complex ones, depending on the associations of the word.

The investigations of many authors, among them Mohr, Hoff, Sahlgreen, Heilig, Glasser, Bekhterev, Sreznewsky, Hakkebush, Finne, Podjapolsky, Shazillo, Abramow, Istomin, Topilsky, Sumbaew, Bahtiarow, Tzinkin and myself, have shown the kind of physiological and biochemical changes produced in the organism under the influence of verbal stimuli. These invariably produce a positive or negative response of the nervous system. This statement may be illustrated by the following words of Loeb: "Ideas [i.e., words (author)] may produce the same influence on men as acids produce on some animals; while raising the sensitiveness toward definite stimuli, they can evoke purposive motor reactions or actions of the kind of tropisms." This statement is borne out by physiology and the experimentation of the above authors whose research receives its scientific interpretation from the Pavlov-Bekhterev doctrine.

It is scarcely worth mentioning that the animal brain as conceived at present represents a storage place for transformed energies as well as organically fixed impressions. These impressions, whether innate or received during the individual's life, are apt to break through at any moment, given certain favorable conditions. At the end of the last century, Hering, Forel, Ribot, and other authors had already mentioned this fact, thus breaking through the wall of subjective psychology and transferring the interpretation of psychological phenomena into the physiological field. At the present time this conception is strengthened by data from clinical and laboratory research.

As Pavlov's doctrine states, the brain cortex presents a picture of motley mosaics of excited and inhibited areas more or less ready to respond in accordance with the laws of reflexology. Each excitation, after reaching the brain surface and connecting with the previous engrams, thus forming a more or less unified cell group, remains in a latent state until a favorable moment when it is activated by means of the conditioned reflex mechanism.

Thus one of the basic life processes of the simple and complex animal organism, the process of reproduction, which is considered by Bekhterev to be one of the main principles underlying the conditioned activity of the organism, is effected. "Each associative reflex," says Bekhterev, "once developeed, does not disappear completely, but leaves behind a trace in the nervous centers. This reflex tends toward reproduction in a similar form under the influence of the same kind of stimulus or another stimulus associated with the first one, though the last be fainter than the previous one." Laboratory experiments have shown that the established conditioned reflex which has suffered extinction reappears later under some conditions. The same holds true for more complex and varied groups of conditioned reactions which constitute the contents of a given personality. If a conditioned stimulus can call forth an elementary conditioned reflex, the same stimulus can bring to life a large complex of chain reflexes with which it is associated in some way. A special activity can in this way be ascribed to the word which is a complex conditioned reflex stimulus calling forth multiple complex reactions. Thus the complex of the associated engrams embedded mosaically in the brain cortex, and inhibited by the superposing and newly acquired ones, may be reinstated and reproduced with all the accompanying physical and chemical changes.

On the basis of these theoretical data we can state that *the suggestion of previous ages brings forth a real organic reproduction of the engrams, the formation of which belongs to the earlier periods of the individual's life.*

A series of questions arises. How can we explain that the large quantity of organic traces are more easily revived in the state of inhibition of the brain cortex during hypnotic sleep? How can we explain why such vivid, isolated engravings from the time of childhood and less remote periods of an adult's life can be easily recalled in the hypnotic state and only rarely in a waking state? (In two of our subjects, N. D. and A., the suggestion could not be performed in a waking state, nor could Kraft-Ebing obtain it with his subject.) How can the fact that the whole experiment is carried out in so short a time be explained? How is it that after the experiments the subjects felt even stronger and more rested than before? Why do these experiments have no harmful effect on the integrity of the personality and its complicated associative activity? These facts are well known to anyone who has had the opportunity to carry on such experiments. Neither we nor any of the hypnologists have ever observed a transformation of the subject into an automaton with a loss of will power and disintegration of personality as the result of the above experiments, a state which the adherents of the Charcot hypnological doctrine believed to result.

Again it should be emphasized that subjective psychology "with its highly conventional and approximate notions of the nervous system" (Pavlov) does not suffice to explain the above questions. Their interpretation rests with the modern "true physiology of the brain" which alone provides us with an explanation of the mechanisms of the higher processes, based on laboratory experiments on animals as well as on humans.

From this we learn that the sources of all the mental phenomena of an individual's life are basic and associated stimuli of varied character which penetrate into the brain cortex chiefly from the outside world. Among these associated stimuli the most important for man is the verbal stimulus. This stimulus, in connection with the individual conditioned physiological mosaics and with its own contents, produces a varied action in different areas of the brain cortex. In connection with past experience, that is, concurrence with other stimuli, positive and negative reflexes, habits, customs, and trends are developed. A repeated stimulus reproduces the same reaction under conditions of varying degrees of importance of inhibition of the brain cortex as a whole, each corresponding part of which is ready to respond to signals of different content. Not all of these signals are able to bring about the varied reactions easily because of the two parallel processes of stimulation and inhibition which are occurring simultaneously, especially in the case of so complex a stimulus as the word. Depending on its content the word may produce within the cortex a series of concurrent foci of excitation and inhibition. The results of the struggle between them and the time it lasts depend upon the conditions of the individual's past, and on the conditions of the given moment, as well as upon the stability of the positive and the depth of the parallelly existing negative conditioned responses. The speed and stability of the processes are also influenced by other conditions. Some of these are constant, as those depending upon the constitution and the type of nervous system; others are temporary, as those influencing the formation of new and the inhibition of old connections in the brain cortex. The basic reflexological law of differentiation plays an important rôle in this respect, in connection with the acquired readiness of the nervous system to react in a strictly determined direction to a definite stimulus, thus securing a ready realization of the positive and negative reactive processes.

Especially favorable conditions in this respect are obtained during the conditioned reflex, partial, sleeping inhibition which constitutes the hypnotic state in which there is only one waking, or non-inhibited area, usually in the auditory region, which favors a strictly differentiated associated reflex to auditory, verbal, stimuli. This waking area, "the watch-point," according to the terminology of Pavlov's laboratory, allows a stimulus of this sort to reach any cortical region and evoke simple and complex reproductive reactions. Under conditions of general inhibition these reactions are unimpeded and quickly realized. The complex stimulus, "you are six years old," in a purely reflex manner brings into life a whole constellation of engrams acquired by the subject at that age. This revival proves to be so vivid that after awakening the wide-awake functionally powerful cell groups dominate the "sleeping" cells of the present. In other words, the verbal stimulus establishes the dominance of a definite, limited content which determines the subject's behavior. It should be added that the cell groups containing the engrams of the age of six were in a state of inactivity during the subject's subsequent life, although ready to react with renewed vigor. At the moment when the reaction develops, the cell groups of the present are still inhibited, inactive, resting, in virtue of a negative induction. This is another manifestation of the principle of the rational equilibrium established "between the process of the organ's reserve substances during their activity and the restitution of these substances in the course of the organ's rest" (Pavlov). Thus the feeling of briskness which always accompanies the awakening of our subjects after the experiment can be explained. In "going back to childhood" they retreat from the fatigue of the present.

The quickness of transition from one age to another can be explained by the conditioned reflex mechanism of all the processes of the organism, including the process of the development of the conditioned reflex cell reaction (Metalnikow, Paris; Podkopaiew, Leningrad). The validity of this statement is strengthened by the weight of steadily accumulating experimental data from the adherents of the Pavlov and Bekhterev schools. The absence of fatigue during our experiments may be explained by the balance between the processes of stimulation and inhibition, that is, inactivity with the aim of the "most differentiated adaptation" of the organism. From this point of view we may consider these experiments by no means harmful, but even useful to the individual, a fact which we have established empirically. The argument as to the harm which can be done to the individual by way of weakening, "dissociating" the personality, and establishing a "slavish" automatism may be absolutely repudiated by taking into account the nervous system types, the cortex mosaic, as well as the mode of reaction realization and the manifestation of habits of the conditioned reflex type. It is well known that every individual is not able to give so lasting, isolated, and complex a reaction. When it does take place, we may assume a lability of the nervous system, normal for the given individual, as well as an acquired readiness for a differentiated reaction.

Do our data and considerations suffice to elucidate the problem in question? If they be considered as giving no exhaustive evidence, they indicate, at any rate, the method by which some phenomena, as yet unsufficiently clear, can be explained. No doubt, proofs by means of reactions of a more objective character would be desirable. At present we are starting, with Professor A. R. Luria and Dr. M. S. Lebedinsky, a series of investigations which ought to provide more objective proofs in support of the reality of the phenomena of regression to previous ages.

The present paper gives only a very schematic outline of the problem, but this scheme, constructed on the basis of the modern theory of the higher activities, enables us to give an objective, physiological interpretation to phenomena which the subjective psychology could never interpret.

Note to the English translation:

We wish to emphasize the importance of the hypno-suggestive method in studying the personality. The facts presented in this paper show the possibility of the investigation of personality in its successive, chronological development by means of an artificial transferring of the individual to the remotest periods of his life. This method, as indicated by our preliminary experiences, gives us the possibility of following the successive formation and development of new reactions, new habits, new attitudes, and the like.

SUR LA PREUVE OBJECTIVE DE LA REGRESSION EXPÉRIMENTALE D'ÂGE DE LA PERSONNALITÉ

(Résumé)

La régression de la personnalité d'un adulte à l'enfance comme résultat d'une suggestion verbale donnée pendant l'état hypnotique est un phénomène qui n'a reçu une interprétation satisfaisante à cause de l'insuffisance de la psychologie subjective à expliquer les processus en jeu. On se sert de méthodes objectives pour rendre clair le problème et pour établir la réalité du phénomène.

On a employé trois sujets, âgés de 20, de 34, et de 39 ans, dont on n'a pu classifier aucun comme "hystérique." Comme résultat de la suggestion hypnotique chaque sujet a montré des régressions aux âges de 4, de 6, et de 10 ans. On a testé les sujets au moyen de la méthode Binet-Simon à chaque niveau d'âge, et dans tous les cas, sauf l'un où le sujet a montré un développement précoce, l'âge mental obtenu a été normal pour un enfant de cet âge. Le comportement a été conforme à l'âge suggéré dans tous les

cas. Des suggestions répétées du même âge ont montré une unité et une stabilité des réponses aux mêmes stimuli, ainsi que l'identité du caractère de l'écriture. On offre des témoignages contre la possibilité de simulation par let sujet.

On offre une explication du phénomène en termes du type de réflexologie Pavlov-Bechterew, basée sur l'activité conditionnelle de l'organisme. Pour l'homme, le mot est un stimulus réflexe conditionnel complexe lequel peut dans l'état hypnotique, et moins facilement dans l'état de réveil, causer une vraie reproduction organique d'engrammes formés antérieurement dans la vie de l'individu, tandis que les engrammes du présent sont inhibités. On appuie sur l'importance de la suggestion hypnotique comme méthode pour étudier la formation de réactions et d'habitudes nouvelles, etc.

PLATONOW

ÜBER DIE OBJEKTIVE BEWEISUNG DER EXPERIMENTELLEN KRONOLOGISCHEN RÜCKBILDUNG DER PERSÖNLICHKEIT

(Referat)

Der Rückgang der Persönlichkeit eines Erwachsenen zur Kindheit, als Folge einer im Zustand der Hypnose empfangenen Suggestion, ist eine Erscheinung die, infolge der Unzulänglichkeit der subjektiven Psychologie zur Erklärung der in Einspruch genommenen Vorgänge, bis jetzt noch keine befriedigende Deutung erfahren hat. Hier hat man, unter Verwendung objektiver Verfahren, versucht, die Aufgabe zu behellen und die Echtheit der Erscheinung festzustellen.

Es dienten drei Versuchspersonen, von denen keine als hysterisch zu bezeichnen war. Sie waren respektiv 20, 34, und 39 Jahre alt. Durch Suggestion unter Hypnose erhielt man bei jeder Versuchsperson Rückbildungen [regressions] zu den Altern von 4, 6, und 10 Jahren. Auf jedem Altersniveau wurden die Versuchspersonen mit der Methode Binet-Simon geprüft, und in allen Fällen, mit der Ausnahme eines Falles in dem eine frühreife Entwicklung bestand, war die erhaltene "geistige Alterszahl" [mental age] die für ein Kind in diesem Alter normale. Das Benehmen [behavior] stimmte in allen Fällen mit dem suggerierten Alter überein. Durch wiederholte Suggerierungen des selben Alters wurde Einheitlichkeit und Beständigkeit der Reaktionen auf den selben Reiz und auch Identität der Handschriftform [character of handwriting] offenbart. Es wird Beweis gegeben dafür, dass Simulierung durch die Versuchsperson nicht möglich gewesen war.

Es wird eine Erklärung der Erscheinung dargeboten auf Basis der Pavlov-Bechterewschen Reflexologie, die auf der bedingten Tätigkeit [conditioned activity] des Organismus gegründet ist. Für den Mensch ist das Wort ein komplizierter bedingter reflexiver Reiz der im hypnotischen Zustand, und auch, obwohl nicht so leicht, im wachenden Zustand, eine echte organische Wiederhervorbringung [reproduction] früher im Leben des Individuums formierter Engramme erzeugen kann, während die Engramme der Gegenwart gehemmt sind. Die Wichtigkeit der hypnotischen Suggestion als Methode zur Untersuchung der Bildung neuer Reaktionen, Gewohnheiten, u.s.w. wird betont.

PLATONOW

SHORT ARTICLES AND NOTES

MODIFICATION OF REFLEXES AND CONDITIONED REACTIONS

Ernest R. Hilgard

When one stimulus regularly precedes another numerous modifications in response may occur. The writer has studied the situation in which a light precedes a sound, first from the point of view of conditioning (3), and, second, from the point of view of reinforcement and inhibition in relation to time interval (4). In both cases, eyelid movements were the measured responses. It is the purpose of the present note to add new data from a subject participating in both phases of the experiment and to call attention to some of the interrelationships revealed in these studies.[1]

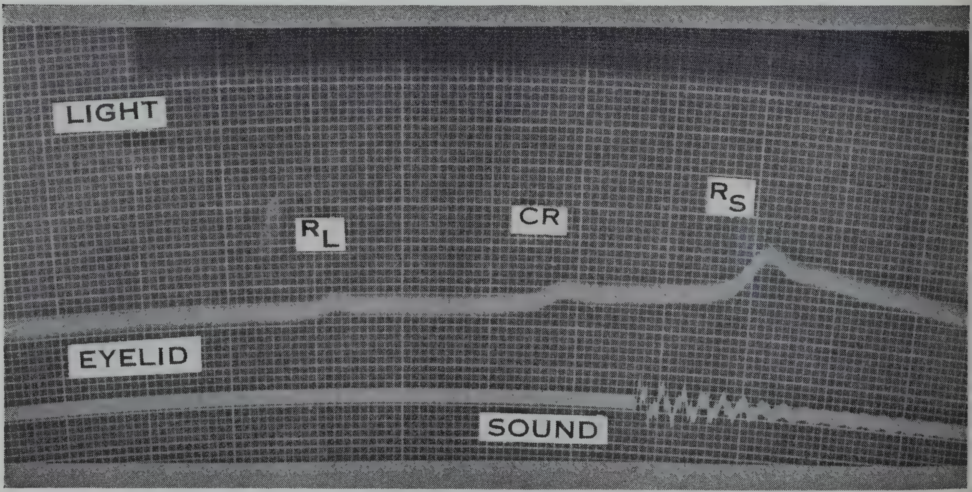

FIGURE 1

Record of Reflex to Light, Conditioned Reaction, and Reflex to Sound

The light stimulus shows as a darkening at the top of the record, the sound as a break in the lower line. Time lines 5 σ, emphasized lines 50 σ. A rise in the eyelid shadow indicates lid closure. The first rise is a reflex to light (R_L), latency 85 σ. The second rise is a conditioned reaction to light (*CR*), latency 185 σ. The normal latency of the reflex to sound (R_S) is 40 σ. Subject B_1.

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[1] This note is presented at the suggestion of Dr. Clark L. Hull.

The specimen record of Figure 1 shows the responses studied. The three reactions on the record are, from left to right, the natural reflex to light (R_L), the conditioned reaction to light (CR), and the unconditioned reflex to sound (R_S). The reflexes to light and to sound (R_L and R_S) are present at the first occurrence of the stimuli; the conditioned reaction (CR) develops only after repetition. This report is concerned solely with the three kinds of response represented on this record.

1. *The Rate of Development of the Conditioned Reaction.* Because of the appearance of the conditioned reaction as an anticipatory response (CR of Figure 1) it is possible to study its amplitude on records throughout the practice, *without introducing test trials.* Practice curves can be constructed from the mean amplitude of the conditioned response in successive practice periods. The curve is usually positively accelerated, as shown in curve *A* of Figure 2. This curve is plotted from the records of Subject F of the conditioning study (3, p. 14). What happens after the period of positive acceleration subsides is conjectural. The curve is bound to become negatively accelerated before maximum closure is reached, but the facts indicate that it may fall off earlier. Curve *B* of Figure 2 is plotted from the records of Subject B_1, here presented for the first time. The points are the means of successive 10 responses, there being 50 double stimulations (reinforcements, in Pavlov's terminology) per day. The curve shows the initial positive acceleration of curve *A,* but after a period of irregular amplitude, the conditioned reaction falls off to zero, *in spite of continued double stimulation.* Pavlov reports the wearing-out of conditioned reflexes after continued reinforcement (7, pp. 234 ff.), and similar effects have been reported by others, e.g., Wendt (11), Wolfle (12), Newhall and Sears (6).

The conditions of decrement with practice should be studied. If acquir-

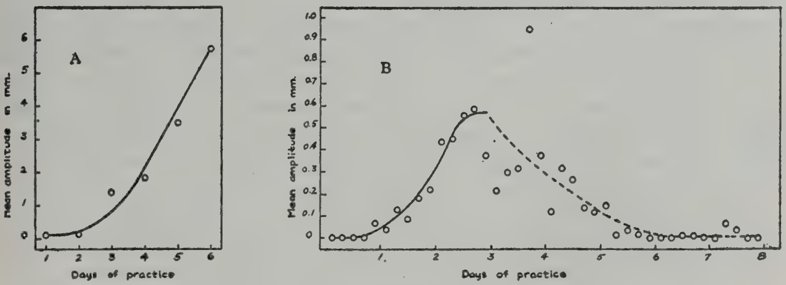

FIGURE 2

The Development of Conditioned Reactions during Successive Practice Sessions

A. Subject F. The points are averages of c. 36 double stimulations per day.
B. Subject B_1. The points are averages of successive 10 records. There were 50 double stimulations per day.

TABLE 1

Amplitude of First Three Reactions in an Extinction Series
(5 subjects)

	Successive presentations of the light alone 1	2	3
Amplitude of conditioned response in mm.	n 41 Mean 7.7 S.D. 9.4	n 41 Mean 2.5 S.D. 2.2	n 41 Mean 4.3 S.D. 5.9
Number of times each amplitude is greatest of the three	23 (56.1%)	10 (24.4%)	8 (19.5%)

ing habits with repetition is analogous to the development of conditioned reactions, this falling-off of conditioning with practice may be the analogue of Dunlap's Beta hypothesis (2).

2. *Is There an Initial Rise in the Curve of Experimental Extinction?* Switzer (9) and Hudgins (5) have found initial rises in the amplitude of successive responses during the first few trials of an extinction series. Data from the conditioning study (3) are considered pertinent when three successive presentations of the conditioned stimulus were given in the midst of a series of double stimulations, or at the end of such a series. Records of 5 subjects are available from 41 series of three (or more) presentations. The comparison of the responses on the three records is given in Table 1. The first response is reliably greater than the second in mean amplitude, and most frequently had the greatest amplitude of the three responses. The records fail to show an initial rise in the curve of extinction under these particular conditions.

3. *Does the Natural Reflex to the Conditioned Stimulus Disappear during Conditioning?* Pavlov (7, pp. 29 f.) suggests that the original motor reactions to the conditioned stimulus drop out during conditioning, and the point has been emphasized by Razran (8). The reflex to light (R_L) is such a reaction. What happens to it as the conditioned reaction (*CR*) develops? The specimen record of Figure 1 shows that *both* the reflex and conditioned reaction may be present during one trial. Comparisons of the reflex to light and conditioned reactions in early and late stages of conditioning are given for three subjects in Table 2. The reflex to light for Subjects D and F is minimal at the start, and remains essentially unaltered during conditioning. The reflex of Subject B_1, which is of greater original amplitude, shows some reduction during conditioning. This reduction, continuing after conditioning has disappeared, is not to be attributed to the presence of the conditioned reaction. (See Table 4 for a study of the three responses during conditioning of this subject.) The reflex to light continues throughout the conditioning series: this is not what would be expected from the theories of Pavlov and Razran.

TABLE 2

AMPLITUDE OF REFLEX TO LIGHT AND CONDITIONED REACTIONS IN EARLY AND LATE STAGES OF PRACTICE

	Amplitude of reflexes and conditioned reactions in mm.					
	Subject D		Subject F		Subject B_1	
	R_L	CR	R_L	CR	R_L	CR
First two practice periods	0.05	0.77	0.12	0.12	1.73	0.08
Two periods of maximum conditioning	0.04	5.11	0.13	4.60	1.25	0.46

TABLE 3

ALTERATION IN AMPLITUDE OF REFLEXES WITH REPETITION OF STIMULI AT TWELVE INTERVALS IN RANDOM ORDER ON TWO DAYS

(10 subjects—60 records each per day)

	Mean amplitude in mm.			
	Day 1	Day 2	Alteration in amplitude Day 1 to Day 2	
Reflexes to sound	14.5	11.9	—2.6	(—18%)
Reflexes to light	0.9	1.3	+0.4	(+44%)
Reflexes to sound reinforced by light	19.0	16.5	—2.5	(—13%)
Reflexes to sound inhibited by light	6.9	6.4	—0.5	(— 7%)

4. *Does Repetition of Two Stimuli at Intervals Favoring Reinforcement and Inhibition Result in Negative Adaptation?* In the reinforcement-inhibition study (4) reactions were secured on two days, but results by days were not presented separately. The alteration in average amplitude from one day to the next is given in Table 3. All reactions except the reflex to light show a decrement.[2] This is in disagreement with the results of Cohen (1), who found no negative adaptation when stimuli were presented with intervals in random order. The increase in mean amplitude of the reflex to light is probably a result of conditioning of the type which Wendt describes as the

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[2] The facilitation-inhibition curves of Telford and Anderson (10), which appeared after my paper (4) was in process of publication, should be added to the family of curves treated there. These investigators also used the eyelid reflex, but they worked at intervals of from ½ to 6 seconds, much longer than those used in the writer's study of reinforcement and inhibition. Decrement in response in their successive series (10, p. 248) is, partly at least, a function of reaction to instructions. Their results for refractoriness at longer intervals are to be compared with Cohen's (1, 1*a*).

sensitizing of a reflex (11, p. 83). It corresponds to the short-latency conditioning of Subject A previously reported (3, pp. 27f.).

Subject B_1, whose responses were treated in the reinforcement-inhibition study, participated later in an extended series of sessions. For eight days, stimuli were presented at an interval of 300 σ, light preceding sound. At this interval the light has a marked inhibitory effect on the reflex wink to sound. It was at this interval, and during these sessions, that the conditioned reactions plotted as curve *B*, Figure 2, appeared. After a rest of four days, there began another series lasting eight days during which the light preceded the sound by 50 σ, an interval favoring reinforcement. The responses during these two 8-day periods are summarized in Table 4. There were 50 trials per day with double stimuli; each entry in the table covers two days.

Several observations may be made regarding the data of Table 4. During the 300-σ sessions, the conditioned reaction developed and decayed. Both the reflex to light and the reflex to sound showed successive decrement. During the 50-σ sessions, there was no recurrence of the conditioned reaction. The responses to sound were being greatly augmented by light. The reflex to light showed decrement, although the level of reaction was above the average of the 300-σ sessions. The reflex to sound alone showed an *increase* during the first 300 trials of the 50-σ series. This is an added

TABLE 4

ALTERATION OF REFLEXES BY REPETITION OF STIMULI AT STANDARD INTERVALS (Subject B_1)

	Mean amplitude of lid motion in mm.			
	Reflex to light	Conditioned reaction to light	Reflex to sound	Reflex to sound preceded by light
Light preceded sound by 300 σ				
Trials 1-100	1.7	0.08	—	5.2 (inhibited)
Trials 101-200	1.3	0.46	—	4.5 "
Trials 201-300	1.0	0.12	—	3.6 "
Trials 301-400	0.6	0.01	—	2.2 "
Light preceded sound by 50 σ				
Trials 1-100	1.9	—	7.2	17.2 (reinforced)
Trials 101-200	1.5	—	8.6	15.9 "
Trials 201-300	1.4	—	9.1	20.4 "
Trials 301-400	1.1	—	8.6	18.8 "

The trials were given 50 per day. The series at 50 σ followed the series at 300 σ by four days. The reflex to sound alone was not sampled during the 300-σ series.

illustration of the sensitization of a reflex through double stimulation. A similar sensitization of the reflex to sound occurred in the reactions of Subject D (3, pp. 21f.) at the same time interval between light and sound. The analysis of the conditions of reinforcement show that at 50 σ the augmented response begins as a reflex to sound, and is continued as a reflex to light (4, p. 99). Even though the light precedes the sound in time, the *reflex* to sound comes earlier than the reflex to light, and this is a case of forward conditioning. If these findings should be confirmed, they would show that forward conditioning has an advantage over backward conditioning *at very fine time intervals,* much too short to be discriminated by the subject.

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THE EFFECT OF AN UPWARD STRESS UPON THE GEOTROPIC ORIENTATION OF YOUNG GUINEA PIGS

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Experiments on the geotropic orientation of various mammals (1, 5, 7) have shown that for a number of different forms the angle of orientation θ is very nearly proportional to the logarithm of the sin of the angle of inclination (α). Crozier and Pincus (2) have suggested that the magnitude of the angle of inclination θ is determined by the number of tension receptors excited by the gravitational stimulus. This notion is in part based upon some experiments conducted on rats, orienting upon an inclined plane, under the effect of a small mass attached to the center of the back. The results show that the magnitudes of θ increased in a regular manner at the low values of α. Assuming that the additional mass brings into play a group of tension receptors which would otherwise not be excited at these low slopes in the absence of the added load, the results are in accord with the hypothesis. Upton (6) conducted a similar experiment by determining the effect of adding a constant relative weight (10% of the body weight) to the young guinea pig. He found that the magnitudes of the angles of orientation were increased, but that the general functional relationship between the angles of orientation (θ) and the angles of inclination (α) was maintained.

The present experiment was conceived as a further test of the hypothesis. It was assumed that if the gravitational stress were to be *decreased* symmetrically the angles of orientation (θ) should become smaller. An attempt was made to lessen the pull of the animal's mass upon its supporting musculature by attaching a hydrogen-filled balloon to the animal's back; but actually this introduces complex redistributions of gravitationally induced stresses.

The plane used in this experiment was 3 ft. wide and 4 ft. long, and was covered with a coarse wire-mesh screen which enabled the animals to secure a good foothold upon the plane. The slope of the plane could be increased or decreased by a rope and pulley arrangement. Luminous spots were placed at the ends of coordinate lines upon the plane and a luminous marker was also attached to the animal's back. All observations were recorded in complete darkness. Two strips of adhesive tape were wound around the animal's body and the balloon was attached to a string slipped under the two pieces of tape. When suspended by this string the animal remained in a balanced position, that is, its body-axis remained parallel to the ground. The balloon was so attached that it would pull directly over the central axis of the animal's body. The volume of the balloon was so adjusted that it created a pull equal to about 10% of the guinea pig's body weight. The diffusion of the gas through the rubber balloon was relatively small, leaving the upward pull on the animal's body practically constant during the experiment.

Table 1 presents the data upon the orientation of young guinea pigs with balloons attached exerting an average pull equal to 10.4% of the animal's body weight. Because the animals changed rapidly in weight each animal was weighed daily, just prior to the experiment, and the volume of the balloon was so adjusted that an average pull of about 10% of the body weight was maintained throughout the course of the experiment. Three guinea pigs (three days old at the beginning of the experiment) were used.

Table 2 shows the data upon the orientation of young guinea pigs with balloons attached exerting an average pull equal to 20.2% of the animal's body weight. N=3, n=30.

Figure 1 shows the relationship between θ and log sin α for the group of guinea pigs where the pull of the balloon was equal to about 10% of the body weight. [The angles of orientation (θ) for this group are repre-

TABLE 1

N=3 (number of individuals)

n=30 [total number of trials for the 3 individuals at each slope (α)]

α	θ	P.E.
degrees	degrees	degrees
10	53.5	1.15
15	51.6	0.75
20	60.8	0.89
25	59.1	0.98
30	61.6	0.61
35	66.3	1.07
40	68.6	0.63
45	70.6	0.53
50	71.4	0.55
55	72.6	0.80
60	77.5	0.46

TABLE 2

α	θ	P.E.
degrees	degrees	degrees
10	69.0	0.92
15	67.4	0.94
20	61.1	0.69
25	61.2	0.55
30	65.4	0.86
35	70.3	0.79
40	67.3	0.79
45	68.1	0.49
50	68.0	0.55
55	73.2	0.61
60	72.9	0.52

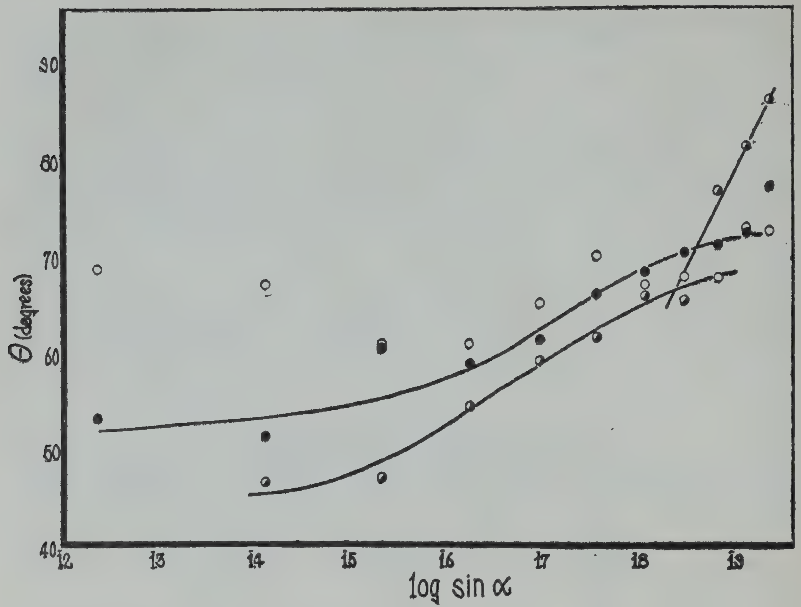

FIGURE 1

The black dots represent the θ — log sin α values for the guinea pigs in Group 1 (pull of the balloon is equal to 10.4% of the body weight); the white dots represent the θ — log sin α values for the guinea pigs in Group 2 (pull of the balloon is equal to 20.2% of the body weight). For purposes of comparison the θ — log sin α values for a group of guinea pigs (without weights or balloons) are represented in Figure 1 by the half shaded dots. These latter values are from an experiment conducted by Upton (5). For the two groups of guinea pigs of this experiment the angles of orientation (θ) are significantly higher for all slopes of surface between α=10 degrees and α=50 degrees. The minimal threshold slope (α) for the guinea pigs with balloons attached has been decreased.

sented by the black circles.] The sigmoid relationship of θ to α is retained, but the magnitudes of the θ values have *increased* throughout the entire range of slopes of surface. In the second experiment, where the pull of the balloon was equal to about 20% of the animal's body weight, the magnitudes of the angles of orientation (θ) have again increased, but the sigmoid relationship between θ and log sin α has been destroyed. It seems probable that a vertically upward pull of this strength and character vitiates the progressively increasing differences in stimulation induced by the gravitational stimulus in the two sides of the body. Hence, although the magnitudes of θ are higher, there is no gradual progression as the α values are increased.

In Figure 1 it is interesting to note that the break in the curve which occurs in the case of guinea pigs and adult rats (5, 7) has practically disappeared in the curve representing the orientation of guinea pigs with balloons attached. This is to be attributed to the elimination of the jumping movements that normally occur in the case of guinea pigs and adult rats at inclinations of the plane above $\alpha=45$ degrees and 55 degrees respectively (5, 7). To climb effectively at these slopes the animal ordinarily must adopt a new mode of progression, jumping, and since this results in a different distribution of the muscular tensions it is no doubt responsible for the change in the relation between θ and log sin α. In this experiment the upward pull of the balloon eliminated all jumping movements. In Figure 1 there is no break in the curve expressing the relationship between θ and log sin α, except for the value at the slope where $\alpha=60$ degrees. But at this point the animal's movements were precarious because of the great amount of slipping which occurred.

Tables 1 and 2 show that the P.E. of mean θ does not decrease with a consistency that is characteristic of the orientation of guinea pigs and rats (with or without weights) (1, 2, 5, 6, 7), where the P.E. of mean θ decreases as log sin α increases. The P.E.'s here are in general smaller at the higher slopes than at the lower, but there is a great deal of variability. This is probably due to mechanical difficulties which are unavoidably introduced by the presence and attachment of the balloon. It is impossible to keep the balloon from swaying as the animal climbs, and this introduces a number of uncontrollable variables that no doubt vitiate the usually obtainable ratio between variability of orientation and the slope of the plane.

The results of this experiment might at first appear inconsistent with the idea that the magnitude of the angle of orientation is determined by the number of receptors excited through gravitational stress. But, as has been pointed out (1, 2, 4) in detailed consideration of the effects of added loads in the case of young rats, attention must be given to the precise manner in which gravitationally induced stresses are brought into play during actual progress of creeping. It is not merely a question of the total downward pull which is significant, but of the proportion of this downwardly directed pull which is borne by the limbs of each side. The basis for interpretation (2) has been that upward orientation is fully effected when, within a threshold difference, the stress supported by the legs on the opposite sides of the body during progression is sensorially equivalent—in terms of the number of these sense-organs and of the frequency of their use. With a balloon centrally attached to the guinea pig's back the situation produced during creeping upon an inclined surface is one in which the upward pull of the balloon tends to throw a greater proportion of the downward drag upon the legs on the upper side, since, as has been described, the animal's axis is horizontal when the feet are not in contact with the substratum (when the guinea pig is held suspended by the balloon cord). This means that with the balloon attached, on a surface of given slope, the guinea pig must turn to a higher θ before it is possible for the gravitationally downward pull to be sensorially equalized in the limbs of the opposite side. Consistent with this, the threshold slope of surface for observable orientation has been definitely decreased.

It follows, also, in the present case, that a general lessening of the maximum angles of orientation at the higher slopes is to be looked for; the evidence for this in the present experiment is given by the absence of the break in the curve for the guinea pigs at $\alpha = 45$ degrees and above.

SUMMARY

Attaching a hydrogen-filled balloon to a young guinea pig causes an increase in the magnitude of the angle of orientation (θ). The relationship between θ and log sin α is maintained, θ being very nearly proportional to the logarithm of sin α. The P.E. of mean θ, however, does not decrease with an increase in the slope of the plane. The loss of this relationship is probably due to the uncontrollable swaying movements of the balloon. The increase in the angle of orientation (θ) may be interpreted in terms of a multiple-receptor hypothesis by assuming that the upward pull of the balloon tends to throw a greater drag upon the legs on the upper side. This means that with the balloon attached, the guinea pig must turn to a higher θ before it is possible for the gravitationally downward pull to be sensorially equalized in the limbs of the opposite side.

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PREDICTING THE COMBINED STANDARD DEVIATION OF TWO COMPARABLE TESTS WHEN EACH HAS BEEN GIVEN TO A DIFFERENT GROUP

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Suppose Group A takes test x_a and Group B takes test x_b. We assume that: (1) tests x_a and x_b are comparable but of different length, (2) the Groups A and B are equivalent in the ability measured by the tests.

We wish to predict the standard deviation on a composite of the two tests, had the composite been given to both groups. The combined standard deviation for the two tests would be

$$\sigma_{(x_a+x_b)} = \sqrt{\sigma^2_{x_a}+\sigma^2_{x_b}+2P_{x_a x_b}} \qquad [1]$$

We have given σ_{x_a} and σ_{x_b}. The problem is to determine $P_{x_a x_b}$, the product moment of the two tests, since the tests have not been given to the same cases.

Now the standard deviation of any test may be expressed as a function of (1) the standard deviations of the individual items of the test and (2) the inter-product moments of the items. Suppose we represent the individual items of the test by $e_1 e_2$, ... Then

$$\sigma^2_{x_a} = \sigma^2_{(e_1+e_2+\cdots+e_a)} \qquad [2]$$

but

$$\sigma^2_{(e_1+e_2+\cdots+e_a)} = \sum_1^a \sigma^2_{e_i} + \sum_1^{a^2-a} P_{e_i e_j} \qquad [3]$$

If we let

$$\frac{\Sigma\sigma^2_{e_i}}{a} = \bar{\sigma}^2_e \qquad [4]$$

and

$$\frac{\Sigma P_{e_i e_j}}{a^2-a} = \bar{P}_{e_i e_j} \qquad [5]$$

then from [2], [3], [4], and [5]

$$\sigma^2_{x_a} = a\bar{\sigma}^2_e + (a^2-a)\bar{P}_{e_i e_j} \qquad [6]$$

Similarly, we have

$$\sigma^2_{x_b} = b\bar{\sigma}^2_e + (b^2-b)\bar{P}_{e_i e_j} \qquad [7]$$

By assumption of comparability, the average item standard deviation of test x_a is equal to that of test x_b. Likewise, the average item inter-product moments of the two tests are equal. From equations [6] and [7] we can determine these average values. Dropping bars, we have

$$\begin{aligned} \sigma^2_{x_a} &= a\sigma^2_e + (a^2-a)P_{e_ie_j} \\ \sigma^2_{x_b} &= b\sigma^2_e + (b^2-b)P_{e_ie_j} \end{aligned} \qquad [8]$$

Solving [8] for $P_{e_ie_j}$ gives

$$P_{e_ie_j} = \frac{a\sigma^2_{x_b} - b\sigma^2_{x_a}}{ab(b-a)} \qquad [9]$$

Equation [9] is the fundamental equation for determining the product moment between two comparable tests of unequal length, each having been given to separate groups of equal ability.

Now the product moment between two tests in terms of the product moments of the items in the tests is

$$P_{(e_1+e_2+\cdots+e_a)(e_1e_2+\cdots+e_b)} = \sum_1^{ab} P_{e_ae_b} \qquad [10]$$

where e_a is, in turn, every item in test x_a and e_b is, in turn, every item in text x_b. But the left-hand side of [10] is merely $P_{x_ax_b}$ and, by assumption of comparability, the item products between tests are the same as the item products within a test, hence

$$P_{x_ax_b} = abP_{e_ie_j} \qquad [11]$$

Substituting the value for $P_{e_ie_j}$ given by [9] in [11], we get

$$P_{x_ax_b} = \frac{a\sigma^2_{x_b} - b\sigma^2_{x_a}}{b-a} \qquad [12]$$

Equation [12] gives the value of the product moment between two comparable tests of unequal lengths when the two tests have been given to separate groups of equal ability. This equation involves only the standard deviations and the number of items in the two tests.

If we substitute the value of $P_{x_ax_b}$ given by [12] in equation [1] we get

$$\sigma_{(x_a+x_b)} = \sqrt{\sigma^2_{x_a} + \sigma^2_{x_b} + 2\frac{a\sigma^2_{x_b} - b\sigma^2_{x_a}}{b-a}}$$

which simplifies to

$$\sigma_{(x_a+x_b)} = \sqrt{\frac{a+b}{a-b}\left(\sigma^2_{x_a} - \sigma^2_{x_b}\right)} \qquad [13]$$

Equation [13] predicts the standard deviation of the combined tests as a function of the standard deviations of the separate tests, and the number of items in each test. Obviously, if the tests are of equal length the solution is indeterminate.

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ELIMINATING HEARING IN EXPERIMENTS ON TACTUAL RECEPTION OF SPEECH

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In our study of vibratory sensitivity and the tactual interpretation of speech and music, we use normal observers for the most part. This, of course, is invariably the case when we wish to compare vibratory with auditory phenomena. The problem of eliminating hearing *completely* has been a matter of concern to every experimenter in the laboratory. It is the purpose of this paper to present experimental data showing the effectiveness with which we have solved this problem. Our apparatus for communication is the teletactor. This consists of a microphone into which the experimenter speaks, an amplifier, and a vibrator upon which the observer rests his fingers to receive through them, by touch, the amplified vibrations of the speaker's voice. The experimenter is in a distant part of the laboratory so that there is no question of the observer's hearing the speaker directly through the air. The only question is whether he may receive some sound stimuli from *the vibrator,* either through the air to his ears, or by bone conduction to the organs of hearing.

To prevent the reception of sound from this source, we have constructed a double concrete box (3 feet cube, outside measurement) lined with sound-absorbing material. The vibrator is suspended from the top of the inside box, and the observer, in order to place his fingers upon it, thrusts his arm into the inner box through a small hole in the wall of each box that has been made to fit the arm. Each opening is encircled with sponge rubber for the purpose of sound insulation when the arm is in position. We have in our laboratory two such boxes, and the experiments to be reported have been conducted with each of them. In the experimental situation, our observers report consistently that they cannot detect any sounds emitting from the box. However, to answer definitely the question whether they receive *subliminal auditory stimuli* from the vibrator within the box and are influenced thereby, we set up the following experiments. As a matter of fact we have repeatedly made experiments of this nature, but not since the construction of the new teletactor and the concrete boxes already described (1, 2).

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[1] The experimental work and computation in connection with this research have been done by Dr. Goodfellow.

The observer was instructed to thrust his arm into the box exactly as in the experimental situation except that he was not to touch the vibrator. He was told that a light would flash about every 10 seconds, and that half of these flashes of light would be accompanied by Victrola music activating the vibrator. He was to guess each time he saw the flash whether or not the vibrator was active within the box. The Victrola record carried a band selection and produced the loudest sounds[2] ever used in our laboratory. Since the observer's hand was not in contact with the vibrator, and the sound-proof box eliminated all sounds of which our observers were conscious, our observer's response, *presumably,* was a pure guess. But if any *subliminal* stimuli were operative, the observers ought to make better than a chance record. In other words, they should prove that their responses were not *absolute* guesses. The following table shows the number of correct responses out of 100 for each of our four observers.

TABLE 1

Observer	Number correct out of 100 trials
R. R.	50
M. I.	43
L. G.	52
M. P.	43
Average	47
Number allowed by chance	50

Since chance would allow 50 correct guesses out of 100 trials we decided, on the basis of these figures, that not even subliminal auditory clues were operative in our research situation. But the question of elimination recurred later. We accordingly altered our technique and went at the experiment again.

The effectiveness of our sound-proof boxes as a preliminary test was measured for different frequency levels by means of the 4-A audiometer connected directly to the in-put transformer of our teletactor amplifier. The observer was instructed as before to thrust his arm into the box but not to touch the instrument. Then by means of the method of right and wrong cases we determined his intensity limen when the sound was produced by the vibrator within the box. Seven persons observed for us, and the following table gives the limen for each observer in decibels above the accepted intensity limen of the normal ear.

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[2] The energy in-put into the vibrator was constantly checked during all of these tests and was always kept well above the maximum used in our research.

TABLE 2

INTENSITY LIMENS OF SEVEN OBSERVERS WHEN SOUND-ELIMINATING APPARATUS IS USED

These figures are in decibels below normal sensitivity

Frequency→ Observer	128	256	512	1024	2048	4096
1	90	95	105	110	105	90
2	75	110	110	130	115	90
3	90	110	125	120	125	95
4	85	100	120	80	100	75
5	90	110	95	120	115	90
6	90	110	115	130	115	100
7	85	95	115	110	105	95
Average	86.4	104.3	112.1	114.2	111.4	90.7
P.E.	–1.5	–2.1	–2.4	–4.8	–2.4	–2.1

These data are a measure of the effectiveness of the sound-proof boxes since they give the differences in sensitivity of the ear when the source of sound is within the box. Viewed from another angle, this means that in the experimental situation our observers have a hearing loss of over 100 decibels for speech frequencies—a loss seldom found among the deaf.

This done, we proceeded with our altered experiment. We used 16 observers and gave each one 200 trials. They were given the same instructions as before, namely, that the light would flash every 10 seconds, and that one-half of the flashes would be accompanied by the experimenter's shouting "Hello" into the microphone. We believed that it would be easier for the observer to detect an auditory stimulus if he knew exactly what to listen for and when to expect it. Then the observer in the exact experimental situation, but without having contact with the vibrator, guessed after each flash of light whether the experimenter had shouted "Hello" or not. The following table, giving each observer's record, shows that, out of 3200 guesses, 51.7% were correct—a chance score beyond question. With this mass of data showing absolutely no evidence of auditory stimuli in spite of the fact that things were weighted in the observer's favor, we are thoroughly satisfied that no *auditory clues* are present in our experimental situation to invalidate our research in tactual sensitivity.

Our next experiment was to test the proposition that possibly some sound may be conducted from the finger-tip up the arm to the organs of hearing (bone-conduction to the inner ear) and that thus our observers are aided

TABLE 3

NUMBER OF CORRECT RESPONSES IN ONE HUNDRED TRIALS TO SUBLIMINAL AUDITORY STIMULI

	Box No. 1			Box No. 2		
Observer	Total no. correct responses	Correct responses when sound from teletactor accompanied light	Correct responses when no sound from teletactor accompanied light	Total no. correct responses	Correct responses when sound from teletactor accompanied light	Correct responses when no sound from teletactor accompanied light
1	54	23	31	62	30	32
2	72	31	41	74	27	47
3	47	23	24	51	25	26
4	46	10	36	52	7	45
5	50	2	48	47	6	41
6	58	26	32	56	13	43
7	33	0	33	63	32	31
8	54	8	46	48	12	36
9	50	29	21	47	21	26
10	50	9	41	57	8	49
11	52	18	34	56	8	48
12	50	28	22	50	32	18
13	50	25	25	50	10	40
14	56	22	34	62	22	40
15	42	8	36	38	10	28
16	41	18	23	38	17	21
Average	50.3	17.5	33.0	53.1	17.5	35.6
P.E.	–1.6			–1.8		
Number allowed by chance	50			50		

by auditory cues in our tactual experiments. We secured 14 observers who had had no previous experience with the teletactor. They were told that a series of Victrola records containing five vowel sounds and repeated in random order would be played over the teletactor and they were to determine by the tactual pattern which of the five vowel sounds was spoken. The records were made from the speech of an instructor in the School of Speech. Each sound was repeated twice at each presentation, and 200 presentations were made in random order and approximately 10 seconds apart. Before the experiment, each observer was allowed to hear a sample set of the vowel sounds so as to be sure he knew the exact sounds to be given in the tactual situation and the quality of the speaker's voice. If there were any conduction of sound to the auditory organs, a naïve observer ought to be able to identify more than a chance number of vowel sounds. Table 4 shows that, out of 2800 trials, 22% were correct. Since chance would allow 20% correct responses, this may be unquestionably taken as a result of chance alone, and the possibility of invalidation of our research on tactual sensitivity due to cues received through bone conduction may be permanently dismissed.

TABLE 4

NUMBER OF CORRECT IDENTIFICATIONS OF FIVE VOWEL SOUNDS IN ONE HUNDRED TRIALS FOR FOURTEEN NAÏVE OBSERVERS

Observer	Box No. 1	Box. No. 2
1	21*	26
2	27*	24
3	34	14*
4	25*	35
5	29	18*
6	18	20*
7	24*	25
8	30*	21
9	24	16
10	22	20*
11	20	15*
12	15*	20
13	20	19*
14	15*	20
Average	23.1–1.1	20.9–1.0
Number allowed by chance	20	20

*Indicates box used first.

We have shown that normal observers using our sound-eliminating apparatus sustain a hearing loss of over 100 decibels, a loss too great to be measured by the ordinary audiometer for the measurement of the degree of deafness; secondly, that out of 3200 trials, 16 observers were entirely unable to guess when a sound was produced, even when they knew the exact sound to expect and exactly when to expect it; and, finally, that 14 observers making an aggregate of 2800 attempts to identify five vowel sounds by touch failed completely.

In conclusion we state again that experimental evidence demonstrates emphatically and unequivocally that subliminal auditory clues, either by direct hearing or by means of bone conduction, are not operative in our experimental situation, and that therefore our research in vibratory and tactual sensitivity is positively unaffected by the sense of hearing.

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THE CORTICAL INTERPRETATION OF CONDITIONING

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Many misconceptions of the conditioned response are the result of a narrow or biased viewpoint, which can be avoided only by a broader comparative approach. Even here, however, we often find a restricted and totally inadequate account. Comparative psychology involves the comparison of the behavior of all living things. In this sense it is general or evolutionary psychology. Three general viewpoints or approaches may be differentiated. Let us briefly consider each in turn, especially with regard to the conditioning process.

The first or "anthropocentric" view, as its name implies, makes man the center of reference and ultimate interest. According to this approach, all material gathered in the animal field is valuable only in so far as it contributes to the understanding of human behavior. Thus conditioning in infrahuman organisms should be studied in order to throw light on the conditioning process in man.

A second and broader view places the interest in each organism as such, regardless of its position on the phyletic scale. Thus exponents of this approach emphatically declare that comparative psychology is intrinsically interesting, and hence cannot be dominated by a single species. This change in viewpoint is not characteristic of psychology alone, but may also be illustrated in the field of biology. Thus the Darwinian movement, originally judged by the bearing it had upon the origin and description of the genus Homo, soon led to the accumulation of such a mass of evidence that man ceased to be the center of reference. Thus, according to this view, the main task of comparative psychology becomes one of cataloguing behavior details, and the conditioned response in each organism should be isolated for separate study.

Many psychologists, in their attempt to counteract the anthropomorphic tendency, have merely collected facts and isolated details, appearing to be entirely oblivious to the implications and relative importance of their data. Thus a third possible approach is one in which the emphasis is placed not on mere man and his conditioned behavior, or on the discrete behavioral data obtained at various phyletic stages, but rather on the *behavior phenomenon* itself. The fact that the phenomena, wherever found, exhibit a common pattern, sequence, or functional characteristic is here taken as the fundamental and unifying principle. The problem then becomes one of studying this pattern, describing its characteristics, conditions, and correlated essentials, and in so doing perhaps striking at the very basis of that ever fascinating phenomenon which has popularly been termed "mind."

Such modifiable or "learned" behavior has frequently come to the attention of the psychologist, biologist, and even the more intelligent layman. Explanations of these phenomena range all the way from attributing the reactions to the workings of animistic transpatial ideas, forces, or mental states, to explanations on the basis of tropistic sensorimotor reactions. Thus the more scientifically inclined workers attempted to bring the phenomena into the laboratory and study them under controlled conditions. Later the psychologist, taking a cue from experimental comparative biology, realized that, although the animal can never give a verbal report of its implicit reactions as can the human, it would nevertheless be a good subject for such research. The situation could be better controlled in the animal experiment, and various intercomparisons could profitably be made among species. Thus the psychologist became interested in the phenomenon *wherever he found it,* and this new all-inclusive comparative approach marked a revolutionary epoch in psychological development.

We may broadly characterize this phenomenon, appearing at all stages of the phyletic scale, as a modification of stimulus-response coordinations. Various terms have been suggested to designate this process, as we shall point out below, among them being the general concepts of modifiability or learning. Recently, however, the more specific term "conditioning" has been employed, and an attempt has been made to subject the conditioning process to a minute analysis. The process is often viewed as the basis of all psychological activities—and possibly of life itself. Any interpretation of the conditioned response will necessarily be questionable unless the phenomenon is viewed in its most inclusive aspects.

The term *conditioning* is used in this paper chiefly because of its wide acceptance among both psychologists and physiologists. The phenomenon to which it applies, however, has been described (and often more adequately) under a variety of terms, such as "redintegration" (Hollingworth), "psychological interaction" (Kantor), "engraphic action" (Semon), "sign-gestalt-expectation" (Tolman), "biosocial response" (Weiss), "combination" (Woodworth), etc. We cannot here enter into a lengthy discussion of the various implications of these views. Our present interest is rather to point out that all are attempts to get at the same fundamentally basic process.

What is a conditioned response? Pavlov describes the *unconditioned reflex* as an innate response which is to be attributed to the inherent organization of the nervous system. Thus such reflexes are essentially invariable responses to appropriate stimuli, and the mechanism involved, the reflex arc, is an inherited structure already elaborated for use. In contrast to such unconditioned reflexes, Pavlov describes the *signal* or *conditioned reflex,* so called because it may be elaborated under certain stimulating conditions. In general, the formation of conditioned reflexes occurs whenever an indifferent (to be conditioned) stimulus, which itself provokes no reflex, is repeatedly presented with another (unconditioned) stimulus which does elicit a reflex. After a number of such repetitions, the originally indifferent stimulus becomes effective in eliciting the same reflex. This process may be summarized as follows: If an indifferent, incidental stimulus be many times presented with one which normally arouses a definite reflex response, subsequent presentation of the incidental stimulus will cause the reflex to appear.

Let us now briefly consider the prevalent view in regard to the nature of the conditioning process. The accepted account is that originally set forth by Pavlov and the Russian school and now uncritically accepted by most psychologists and physiologists. According to this view, the formation of conditioned or "signal" reflexes is a part of the synthesizing activity of the cerebral cortex, and such reflexes are the best sign of the functioning of the cerebral hemispheres. The physical basis is considered to be a process of neural excitation in the cortex, an impulse coming in from the periphery tending to become associated with any area which is in a condition of stimulation. Thus the cerebral cortex not only affords the means of making signal reflexes, but Pavlov definitely states that without the cortex such responses cannot be elicited. Investigators have been quick to point out that the ability to form such reflexes is correlated with the developmental evolution of the brain. In fact, the method is often advocated as an objective means of studying what occurs in the cerebral cortex. Thus the cortex is considered as a vast compound shunt system mediating all learned or conditioned behavior.

The writer would question the so-called cortical basis or interpretation of conditioning. There are two general lines of evidence to date which he would put forward in support of such a contention. These are inferences drawn from general knowledge and previously obtained data in the fields of ontogeny and phylogeny. A third source of evidence forms the basis of a proposed experiment, now in progress in the Columbia Laboratories of Comparative Psychology. Let us briefly enumerate each of these in turn.

The first of these contentions is drawn from the field of *ontogeny.* It is generally agreed that at least during the early months of embryonic life, especially in the higher organisms, the connections within the neopallium and between this and the archipallium and subcephalic structures have not fully developed. A definite maturation period, varying in different species, is required for myelinization of such tracts to take place. On the other hand, the evidence seems to show that conditioned reflexes are formed during the earliest stages of development. In fact, it is frequently argued that it is at just such periods that the nervous system is "plastic," most susceptible to stimulation, and most amenable to change. These two statements appear to be contradictory, for if one accepts the doctrine of retarded neopallic maturation, it is difficult to see why conditioned reflexes are produced so readily in the young organism. This contention at least casts considerable doubt on the statement that the acquisition of conditioned reflexes is a special function of the cerebral cortex.

The second and somewhat more forceful contention is drawn from the field of *phylogeny*. The contention here may be stated in a few words. If conditioning is a function of the cerebral cortex, how does one explain the numerous cases in which this phenomenon has been obtained in lower organisms which have no cortex whatsoever? To hold such a view, one must argue either (1) that such results are not evidence of "true" conditioned reflexes, or (2) that in such cases a nerve net or other neuromechanism mediates and produces the conditioned reflex. It seems almost needless to point out the absurdity of such statements. As to the first argument, we might state that it would be far nearer the facts to argue that the conditioned reflexes obtained in the lower organisms were the only valid ones, since such reflex patterns are relatively simple, definite, consistent, and dependable, especially when compared with those obtained in the higher chordates. The second argument involves an inconsistency in definition. To state definitely that the cerebral cortex is necessary for conditioning and then to attribute the phenomenon to such elementary neural structures is neither good logic nor good psychology. Moreover, when the process is found in unicellular organisms which have no differentiated nervous system whatsoever, such an argument becomes entirely untenable.

One other fact should be mentioned in this connection. Those who contend, as we have shown above, that the ability to form conditioned responses is correlated with the developmental evolution of the cortex have merely pointed to one variable in an increasingly complex system, and have totally neglected the numerous other equally important factors which are also correlated with such an ability. To ascribe such a function to a single feature of this organismic complex is obviously bad logic.

Lastly, we come to the third and perhaps the crucial test of Pavlov's hypothesis, and this forms the basis of a proposed experiment. If conditioning is a function of the cerebral cortex, then it should not appear in a completely decorticated organism. To test this hypothesis, it was decided to extirpate the entire cerebral cortex of the Macacus rhesus monkey and to endeavor to establish the conditioned response postoperatively. Another related problem was to determine whether preoperatively conditioned responses were retained postoperatively.

The rhesus monkey has been chosen for such a study because, if lower forms were employed, the cortex would be relatively poorly developed and no definite conclusions could be drawn. Tilney has classified the genus Pithecus, which includes the macaques, as an "intermediate primate," other members of this group being Papio Cynocephalus, or common dog-headed baboon, and Hylobates Hoolock, or gibbon. The great difficulty of complete cortical extirpation in such a form, however, is to be considered. The writer has secured the cooperation of Dr. S. E. Barrera of the Psychiatric Institute, Columbia Medical Center, and it is hoped that this previously insurmountable difficulty can be overcome.

The problem of stimuli and indicator responses to be employed is also a serious one, since experimental conditions should be optimal for the establishment of the conditioned response. After extensive correspondence and investigation, it was decided to use a tactile stimulus and electric shock as the "conditioned" and "unconditioned" stimuli respectively, and to endeavor to build up a conditioned withdrawal or avoiding reaction of the foreleg to a tactile stimulus applied to the dorsal surface of the body. The apparatus for administration of the latter stimulus was constructed with slight modifications from a tactile stimulator from Pavlov's laboratory in Moscow. Investigations seem to have shown that tactual stimuli are most effective for rapid conditioning in such organisms. If negative results were to be obtained with the above set-up, other stimuli (light, etc.) will be employed. The one restriction, of course, is that the stimulus-response coordination selected must not be mediated by the higher centers which are operatively destroyed or else any possibility of conditioning would be precluded at the outset. Data will also be obtained on a control group of normal animals, so that adequate comparisons can be made in regard to the relative efficiency of the conditioning process under the existing experimental conditions.

The question always remains, of course, as to the possibility of conclusions drawn from negative results when operative conditions are employed. If purely negative results are obtained, no final interpretation is possible, since the absence of the conditioned response may be due to the fact that optimal experimental conditions were not employed, to the loss of the cortex, or to a general bodily disturbance resulting from the operation. The last-named possibility is sometimes excluded in certain cases by showing that a sham operation of the same general type does not result in negative conclusions, but it is doubtful if such could be profitably conducted in this particular case.

If possible, postoperative conditioning tests will be begun only after recovery from all signs of surgical shock or depression. At necropsy, the position and extent of superficial lesions and extirpation will be determined and marked on previously prepared brain diagrams, etc. These preliminary determinations will be further checked by complete microscopic sections and photographs, so as to permit of more accurate delineation of the exact nature and extent of cortical extirpation. It will also be determined if there are chance injuries to the deep-lying structures, such as the corpus striatum, hippocampus, fornix, thalamus, etc. Lashley, in discussing the difficulties of interpretation in studies of brain injuries, lists a number of variables which, because of practical difficulties of technique, are almost impossible of independent control. Among these he includes the following: individual variation in localization, specific shock or diaschisis effects, vicarious function, equipotentiality of parts, mass function, and disturbances of the equilibrium within functional systems. In a study involving complete decortication, however, few if any of these factors would be of serious consequence.

If, however, positive results are obtained, conclusive evidence will be afforded that the cortex is unessential to the formation of the conditioned reaction. This would hold good for the rhesus monkey and all lower forms, although the generalization could not be drawn to include the higher primates and man. If conditioning appears but is less efficient after operation, the above conclusion can still be drawn provided there is no question about the presence of the phenomenon. Conditioning would then best be regarded not as a specialized function of the cerebral cortex, but rather as a generalized function of protoplasm itself. The physiologist has long recognized the elementary function of irritability as basic to protoplasmic structure. Perhaps the phenomenon which we call conditioning is equally fundamental to organismic existence and reaction. Such an interpretation, however, awaits empirical experimental results.

Summary

The writer holds that the problem of conditioning should be regarded in its broadest aspects, which necessitates a comparative approach. After a statement of the fundamental pattern of the conditioned reaction, Pavlov's theoretical interpretation is presented, viz., the assertion that the cortex is basic and essential to the conditioning process. The writer questions this interpretation and puts forward evidence from (1) *ontogeny* (conditioned responses are readily produced in embryonic organisms in which neopallic maturation has not yet occurred) and (2) *phylogeny* (conditioned responses are obtained in lower organisms which have no cortex or differentiated nervous system whatsoever). Lastly, the writer suggests a third and perhaps crucial test of Pavlov's hypothesis: the experiment of extirpating the entire cerebral cortex and endeavoring to establish the conditioned reflex postoperatively. The remainder of the paper is devoted to the description of such an experiment now in progress in the Columbia Laboratories of Comparative Psychology, using the Macacus rhesus monkey, with electric shock and taction as the unconditioned and conditioned stimuli respectively. Various experimental conditions and controls are described, together with possible interpretations of empirical results.

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THE DEWEY DECIMAL SYSTEM AND PSYCHOLOGY

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The most widely used library classification scheme, that devised by Melvil Dewey and first published in 1876, has never adequately considered psychology. At the time of its origin psychology was classed as a minor branch of philosophy, but even on that basis a rather unexplainable division was made, viz., Mind and Body was given class number 130, followed by Philosophic systems as 140, and that in turn by 150, Psychology. With subsequent editions no substantial changes were made and as new fields of inquiry were introduced into the science they were not recognized in the classification as a part of psychology at all. The impracticability of the Dewey scheme for use in psychological collections even as early as 1894 is shown by a comment of Dr. H. C. Warren's published in the *Proceedings of the Thirty-eighth Annual Meeting of the American Psychological Association* (1): "When the Psychological Index was started in 1894, I examined the Dewey System carefully with a view to adopting it. But I found their arrangement even at that time absolutely unsuited to psychology."

The attitude of the Dewey Decimal people has been that any major changes in their scheme were impossible because of the confusion that might arise in libraries and the cost of making changes. This is, perhaps, a logical and justifiable reason for no change. However, at the December, 1930, meetings of the American Psychological Association (1) the Council of Directors "reported the receipt of a request from the Dewey Decimal Classification and Relativ Index for the Appointment of a Committee to make suggestions for a revision prior to March 1." The Council requested an expression from Dr. Warren, the tenor of which is shown in one paragraph: "I doubt whether it is worth while for the American Psychological Association to take an official action. The Dewey people would not for a moment consider any radical revision of their scheme. And it would need thorough rearrangement to fit in with modern psychology. We can't express approval; and if public libraries find the system generally useful, why should we interfere in their business?" At this meeting Dr. Madison Bentley was elected as a committee to confer with the Dewey people and if there seemed to be some hope of success he was authorized to appoint other members on the committee. That Professor Bentley did not find a very ready response is indicated by the fact that no other members of the committee were ever reported as having been appointed. Furthermore, at the 1931 meetings of the Association (2) it was voted that a communication be sent to the Dewey Decimal Classification expressing the belief that recent changes in the psychology section were of service, but again pointing out that fundamental revision was needed.

In the fall of 1932 the 13th edition of the Decimal Classification and Relativ Index appeared under the editorship of Dorkas Fellows, Dr. Melvil Dewey (3) having died in December, 1931. In this edition the classification for psychology has been much expanded, but still remains in the old divisions of 130, Mind and Body, and 150, Psychology. However, there is offered an alternative scheme for psychology tacked on to the end of 159 (which is volition in the old scheme) as 159.9. It is this alternative scheme we are to consider.

This classification is claimed to be not only for library use, but also, to quote from the title page, for "personal use in arranjing for immediate reference books, pamflets, clippings, pictures, manuscript notes and other material." With this in mind one could only expect that the system would adequately serve for a special collection of psychological literature. In previous editions the scheme has not done so; the alternative scheme of the present edition, unless it is half-hearted compromise, should certainly do so. A careful examination of the schedules raises grave doubts.

In the first place what will one do with literature in fields totally omitted. For example, the field of clinical work, psychological, psychiatric, mental hygiene, or child guidance, has an enormous literature. Neither in the complete tables nor in the relative index are these fields mentioned. Mental clinics of any sort are included only under psychoanalysis where they share a long number, 159.964207, with training classes.

Neither, in this alternative scheme, is any provision made for the psychologies of religion, education, crime, language (except in a non-subdivided heading 159.9463, vocal expression, which is to include speech, singing, whistling, etc.), esthetics, or advertising, to mention some of the important fields. Among minor topics for which one searches in vain might be mentioned the organismic school (although endocrinism as a school is given a number), handedness, eye reflexes (except pupillary), general effects of drugs, the glandular system as a reacting mechanism, or apparatus in general. Bibliographies are given no place at all within the psychological scheme, although other collections and history may be fitted in by the use of a general table of "Form divisions."

Omissions are not the only queer things to be noticed. Physiologic psychology, 159.91, is a curious rubric. It includes the anatomy and physiology of the nervous system, but nothing about other parts of the organism. The deviser of this section surely never saw Ladd and Woodworth's *Physiological Psychology* or Troland's *Psychophysiology*. Then to complete the picture Mental Hygiene is included as the third major division! Why it should go here instead of sensation, motor phenomena, animal behavior, or any one of a dozen other headings is a mystery. In practical application this means that works on mental hygiene would be separated the whole length of the library from those most closely related, i.e., abnormal psychology, 159.97. Mental hygienists will find New Thought movements a part of their domain, but nothing said about the influence of environment at home, in school, or at work.

Mental development and capacity and comparative psychology (meaning plant and animal behavior) are combined in number 159.92. This section includes physiognomy divided into phrenology, graphology, and palmistry, each of which are so finely subdivided that their symbols have as many as eight decimal places. The section for plant and animal behavior is not subdivided but note is made that it may be divided like the general scheme. Even this device cannot accommodate such a work as H. C. Bingham's monograph, *Gorillas in a Native Habitat,* nor does it make any provision for a phylogenetic arrangement which Willoughby's (4) classification indicates is useful.

There seems to be confusion in relation to synesthesia. Colored audition goes in 159.931373, but all other types of visual synesthesia go in 159.93771, while figured or geometric audition goes in 159.937713. Also in connection with apparatus used in studying sensation there is duplication. Apparatus for each sense field has a number in 159.93824, but in the sections for each sense is a number for tests and measurements which would include apparatus.

Psychic research, 159.961, has five pages devoted to its subdivisions, or about 9 per cent of the total scheme. Illusion is put under this heading although another section under perception, 159.9373, is reserved for "normal illusions."

Section 159.97, abnormal psychology, is entirely hopeless. Apparently the official classification of mental diseases of the American Psychiatric Association remains unknown to the compiler of the alternative scheme for psychology. In the published schedules the psychopathological effects of alcohol may be found under five different numbers. Dipsomania, kleptomania, pathological lying, etc., are classed as types of moral imbecility under mental deficiency, while modern practice considers these as types of psychoneuroses. Incidentally it is interesting to note that neurasthenia is not included as a psychoneurosis, while epilepsy is.

Practically nothing is said of feeblemindedness. The section mental deficiency includes psychopathic personality, moral imbecility, and intellectual defect. This last has only three divisions—grades, accompanying deformity, accompanying disease and injury. The word feeblemindedness is used only once and then in the English sense of moron. Upon looking for feeblemindedness in the relative index we find:

asylums for	362.3
architecture	725.53
child study	159.9227623624
education of	371.92
libraries for	027.6623
mental derangements	159.973624 (this is for morons)

Not a single mention of diagnosis, social relations, psychobiology, or the many other topics one finds in this large literature.

A third cogent criticism of the system relates to the many cumbersome class numbers. As the alternative scheme is based on 159.9 every number starts off with four digits. With expansion the class numbers get as long as fifteen digits. For example, the eye-movement theory of geometric illusions has number 159.931374150163, while von Kries's duplicity theory has class number 159.93133016472 which is only fourteen digits! Partly controlled association reaction-time is number 159.9383433532, only thirteen digits, but one wonders if there is sufficient literature to make such a symbol justified.

The decimal classification published by Willoughby (4) is an excellent example of the way modern literature might be satisfactorily classified. Checking the Dewey scheme against this much condensed outline we find the omissions earlier mentioned. As Willoughby's scheme was devised by considering actual numbers of titles to be classified it is at once evident that the Dewey omissions are serious.

The classification offered by the Dewey Decimal scheme has never been adequate for psychology. In the attempt to correct this inadequacy the 13th edition offers an alternative scheme supposedly following modern lines of thought. A careful examination of this scheme makes one wonder if it was even seen by a psychologist before being published. We can with certainty say today, as Warren could in 1894, that the system is "absolutely unsuited to psychology."

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A REPETITION OF THORNDIKE'S EXPERIMENT ON IMPROVEMENT IN THE ESTIMATION OF LENGTHS

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Thorndike (2) in 1927 reported that subjects who were informed as to their correctness in estimating lengths of lines improved, whereas subjects who were given no information, aside from the presence of a standard of known length, did not improve. On the basis of this experiment and many others, Thorndike (3) has recently reached the conclusion that "the repeated occurrence of a situation, in and of itself, does not produce adaptive learning."

Although we agree with Thorndike that improvement is not a function of repetition *per se* but a function of conditions mediated by repetition, e.g., consequences of behavior, we believe that he holds a narrow view regarding these effective consequences. For it would appear on the basis of this length-judging experiment that the development of more suitable behavior is solely dependent upon verbal acknowledgment of its correctness. We suggest, however, that the acquisition of more adequate conduct may be self-regulatory. We believe that individuals possess the ability to recognize faulty responses and to correct them on repetition of the situation. Of the elements in the situation or the reacting organism which lead to such self-regulation, we are ignorant. But the hypothesis is consonant with a substantial body of psychological data. Dodge (1), to cite but one example, has found that there is improvement in the exactness with which the eye follows a moving light despite any information from an outside source regarding the correctness of these pursuit movements.

We were prompted, therefore, to repeat Thorndike's experiment because we expected that improvement would be noted in the situation where a standard of known length alone was present.

Procedure. The procedure of our experiment was similar to that used by Thorndike. A group of never more than eight individuals sat at a distance of 6 feet from an easel on which were presented the lengths to be judged. The shelf on which the cards rested was about 3 feet from the floor or at approximately eye-level. The lengths were black strips of the same width (22 mm.) and texture. Twenty-five lengths made up the variable series, there being one such length for each unit from 3 to 27 centimeters. Each strip was separately mounted on a white cardboard background 35″ x 20″. The following directions were given to the subjects.

> "This experiment requires that you judge lengths of strips in centimeters. A series of 25 strips will be exposed one at a time, and after each exposure you are to write down your judgment on the sheet which has been given you. [The data sheets contained four columns of 25 numbers followed by spaces in which the subjects were to record their judgments.] The lengths will all be multiples of 1 centimeter; that is, there will be no fractional lengths. You will have 15 seconds in which to look at the length and make a judgment. Never go back and change a preceding judgment. It is especially important that you do not let your eyes wander to your neighbor's record. The success of this experiment depends largely upon these judgments being your own. [To obviate any cheating, each subject was given high-sided cardboard boxes in which to record his judgments.] There will be four trials, 25 judgments to each trial. At the end of each trial I wish you would fold over the data sheet so you will not be able to see the judgments of the preceding trial." [At this point a strip of cardboard 10 centimeters long was shown to the subjects. They were told its length in centimeters. This was necessary because it was found in preliminary testing that some of the subjects had no idea as to what a centimeter was and hence were inclined to be uncooperative.]

After these instructions the first trial of 25 lengths was run off. The lengths were presented in random order but the same order was used for all subjects. Five variations in the procedure with five different groups of subjects took place during the second and third trials which constituted the training period. One group of subjects gave judgments without the aid of any standard. That is, the procedure was the same throughout the four trials. The second group had one standard of known length with which to compare the variable series. The third group had two such standards, the fourth group three, and the fifth group four. These standards were located to the right of the variable lengths and were not removed during the two trials. When two or more standards were used, the lengths were placed on the same background one above the other in a staggered order. The length of the standard in centimeters was printed by its left end. The lengths of the standards are given in Table 1. They were selected so as to divide the variable series into approximately equal parts. Thus, Group III did not have the standards of Group II plus a new one but three entirely different standards which would fractionate the variable series into three equal parts. After the two training trials the standards were removed and a final test trial made. The order of presentation was identical with that on the first trial although the subjects were not informed of this fact. The entire experiment lasted about forty minutes.

Results. The following analysis of the data was made. The difference

TABLE 1

MEANS AND STANDARD DEVIATIONS OF THE INITIAL AND FINAL TRIALS AND THE PERCENTAGE IMPROVEMENT

Group	Standards	N	M_i	SD_i	M_f	SD_f	D	%I
0	(None)	34	57.94	26.80	62.91	37.86	−4.97	−08.6
I	15	29	57.31	29.89	36.72	17.76	20.60	35.9
II	11, 18	31	57.39	28.41	38.61	16.17	18.80	32.7
III	9, 16, 22	29	57.17	30.97	32.52	9.69	24.65	43.1
IV	7, 12, 17, 22	29	57.10	33.25	34.31	10.89	22.80	39.9

i=initial; f=final; $D=(M_i—M_f)$; $\%I=(D/M_i)$

between the actual length and the judged length was computed in centimeters for each length. These deviations were then summed without regard to sign for the initial and final trials and the difference between these two scores taken to represent the improvement. The percentage improvement was found by taking the difference between the initial and final scores and dividing by the initial score. The results of such an analysis appear in Table 1.

The following facts may be gleaned from this table. (1) Subjects uninformed as to the correctness of their judgments and given no standard with which to compare the variable lengths do not manifest any improvement. (2) Subjects uninformed as to the correctness of their judgments but given a standard for comparison improve in their ability to estimate lengths. (3) The addition of more than one standard is no aid to learning. (4) Variability is reduced as the number of standards is increased.

Upon what feature of the experiment does this improvement depend? The following hypotheses may be considered. (1) Improvement may be a function of the "learning-set" which the subjects adopt. This hypothesis is disproved by the fact that Group 0 did not improve although there is no reason why they should not have had the same "learning-set" as the other four groups. (2) Improvement may be entirely a function of repetition. Group 0 again disproves such a thesis. (3) Improvement may be a function of number of standards. We favored this hypothesis before the experiment and it led us to use more than one standard in the hope of being able to derive an equation

$$\text{Improvement} = F\ (\textit{number of standards}).$$

The results do not bear out our hypothesis. (4) Improvement may be a function of the presence of a standard with no specification as to the number of standards. This hypothesis is in accord with the data.

What is the underlying factor which explains improvement when a standard length is present? We hazard the suggestion that the estimation of length is the estimation of eye-movements made while scanning a given length. Improvement, therefore, would consist in the ability to compare the eye-movements made in looking at the standard and at the variable lengths. We have no data on this point.

Conclusion. We have found in contradiction to results presented by Thorndike that improvement may take place in length-judging even though the subject is not informed as to the correctness of his judgments. This improvement is dependent upon training with a standard of known length, but is not dependent upon the number of such standards presented.

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A REPLY TO PROFESSOR ESPER

Dael L. Wolfle

In a recent article in this journal Professor Esper (2) has stated that a publication of mine (3) "is essentially a repetition of the experiments" which he published in 1925 (1) "with some additional features of method and interpretation whose resemblance to some of those of the present article may seem striking in the absence of any explanation by Mr. Wolfle." The similarity is supposedly due to the fact that "as a student in my laboratory, and in subsequent correspondence, Mr. Wolfle was made acquainted with much of the material here presented" (2, p. 346).

There are several reasons why my work should show a number of points of similarity to that of Professor Esper. These are: (1) we have been working on related topics in the same general field; (2) much of our training was received from, and much of our research directed by, the same men; and (3) the work for my Master's thesis was performed under Professor Esper's direction. This is the most important reason for whatever similarity exists. When I left the University of Washington, Professor Esper urged me to continue working in the same linguistic field as I had been doing under his direction. This I did, and for the next two or three years we maintained an irregular correspondence in which (and in two visits he made to the Ohio State University) I kept him acquainted with what I was doing, and frequently called upon him for advice. Though he was thus aware of the nature of my research, Professor Esper at this time never made the objection that it bore too great a similarity to his own.

In performing the experimental work for the article in question I used different experimental material and worked out a new problem as my own particular research. I continued, however, to use the experimental method which I had used under Professor Esper, and which he first developed.

The greatest point of similarity between our experiments is that I have taken over his already developed technique, almost without modification. However, I have always stated this fact. In the article to which objections are raised I gave Professor Esper credit for the use of the particular method and stated that my report "will illustrate this general method" (3, p. 542). In a later monograph (4) I more fully expressed my gratitude, and again gave credit for the origin and development of problem and method, explicitly stating my indebtedness, and the fact that I had used his technique (4, pp. 5, 7, and 8). The above statements are given as evidence that I have never attempted to conceal the fact that my work has been definitely derived from Professor Esper's, that I have used his experimental technique, and that I am grateful for the training and help I received from him.

As evidence that the research is sufficiently original to merit consideration by itself the following facts may be cited: (1) Both Professor Esper's 1925 monograph (1) and my work were dissertations developed under the direction of the late Professor A. P. Weiss. Had my work been "essentially a repetition" of the earlier experiments it is hardly conceivable that Professor Weiss would have accepted it as "an original contribution," for he was thoroughly conversant with both studies. (2) Both studies were published as *Language Monographs*. It is improbable that the Linguistic Society of America would have published the mere repetition of an experiment which they had already published. Professor G. M. Bolling was editor of that series at the time of both publications and read both manuscripts. It would be difficult to find two more capable judges in this field of the psychology of language than Professors Weiss and Bolling.

While altercations of this nature are always to be regretted, in the present case it has seemed necessary to answer Professor Esper's charges as the lack of a reply might imply their truth.

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APPARATUS

A MODIFICATION OF RANSCHBURG'S EXPOSURE APPARATUS

SIEGEN K. CHOU

All exposure apparatuses are designed for horizontal exposure, because in the Western languages only horizontal alignment is used in printing. The Chinese language is originally a vertical language, although recently horizontal alignment is also used in printing. For technical reasons, the writer designed the quadrant tachistoscope in such a way that the alignment can be changed at will without disturbing other instrumental control during experimentation. This apparatus was designed specifically for the purpose of comparing the relative legibility of vertical versus horizontal alignment.

The advantages of the quadrant tachistoscope are (1) that the opening and closing of the quadrant shutters can be controlled either by the experimenter or by the subject; (2) that the quadrant shutters can be opened either sidewise or in the upward and downward direction as the experimenter requires and this opening can be made very quickly and at will so that both horizontal and vertical materials may be exposed as required in the entire field; (3) that the time of exposure is capable of objective record by means of either a chronoscope or a short-interval stop-watch; and (4) that the changing and releasing of the reading cards are quick and automatic so that a large number of exposures may be made possible during one sitting. At present none of the existing exposure apparatuses fulfills these requirements.

However, in the absence of such a relatively satisfactory instrument for use, many well-known tachistoscopes and bradyscopes may be easily adapted for vertical exposure. In the *drum* type of bradyscope, the subject needs only to change his sitting position in such a way that the axis of the drum will be vertical instead of horizontal before him. Schumann's, Meumann's, and Müller's instruments will all serve this purpose with the single precaution that perhaps the line of sight should be directed perpendicularly downward above the drum in order to insure equal distance to the eye from both the top and the bottom of the oblong window.

The *booklet* type of Baird and Schulze and the *endless-band* type of Wirth, Wohlgemuth, and Esper are also amendable for vertical exposure by furnishing a special cover with the window open in the vertical direction. The details for individual adaptation vary with different instruments. Some may only require turning the cover 90 degrees and some may need an entirely new cover.

The *spring* and *camera shutter* types of tachistoscope are easily adaptable to vertical exposure by simply turning the whole apparatus 90 degrees so that the slit on the shutter will be in the vertical direction. Dodge's *mirror* type of tachistoscope may readily be designed with the exposure field in the form of a square instead of a horizontal oblong field. Structural changes can be easily made by altering the hood openings and the partition openings all into squares. The smoked glass inserted diagonally just back of the hood in the line of vision should also be a square one. Of course the card and card-holder should also be square in shape.

The *fall* type of bradyscope of Minnemann and Kuhlmann and the Wundt *gravity* type of tachistoscope are least amendable for vertical exposure, because simultaneous exposure can be obtained only in the horizontal field

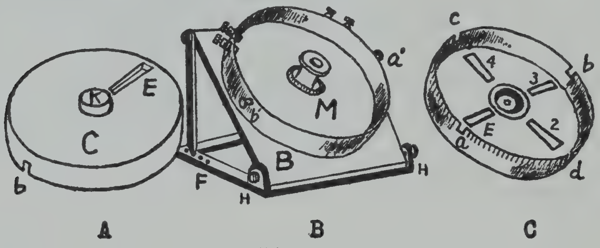

FIGURE 1

A Modification of Ranschburg's Exposure Apparatus

The electric operating mechanism inside *M* in Figure 1B has been omitted.

The exposure aperture, *E,* on the cover cup in Figure 1A may be placed at will on the top, left, or bottom of the center of the cover with respect to the hand knob, *K,* when the lid, *C,* is placed in position above *M* in Figure 1B. This free shifting of the exposure window in various positions around the circumference of the disc is made possible by making the following modifications:

1. Turn the base, *B,* 90 degrees around by refastening the screws at *H* to the other sides of the frame board, *B.* Extra holes need to be drilled.
2. Cut three apertures, *2, 3,* and *4,* on the cover cup as in Figure 1C. Use an extra cover similar to, but larger than, the original cover for covering up the unused apertures. Since the extra cover has only one aperture in it and is larger, it turns on the top of the four-window cover and exposes only one aperture at a time.
3. Cut two more slots, *c* and *d,* similar to *a* and *b,* as in Figure 1C, so that they will also fit the screw nuts *a′* and *b′* on the sides of the apparatus. The original aperture, *E,* will be in any of the four cardinal positions by screwing slot *a, b, c,* or *d* to screws *a′* or *b′.*
4. Ignore entirely the slots on the cover and the screws on the apparatus. Mark the circumference of the cover into degrees and insert a pointer in a convenient place on the outside rim of the apparatus. All intermediate positions of the exposure window may be secured by simply turning the cover in degrees with reference to the indicator.

when either the field or the cover falls by gravity. If the exposure is in the vertical direction, the exposure material will be in serial order from top to bottom when the cover falls and from bottom to top when the field itself moves. Even in the *spring* and *camera shutter* types of tachistoscope, turning the instrument as a whole 90 degrees may appreciably change the tension of the spring and hence the speed of the shutter.

Bradyscopes and tachistoscopes of the *disc* type can be very easily modified for vertical exposure either by turning the whole apparatus around if it is small or by cutting more slits in the cover. In this type of exposure apparatus, the horizontal opening is usually at the right of the cover of the disc. But theoretically it may be placed at the left, at the top, or at the bottom. In fact, reading direction may be in all positions varying in various degrees from the normal vertical or horizontal. For investigating this physical factor on reading efficiency, a turnable cover may be fitted to any disc exposure apparatus so that the opening may be easily set at will in any position with respect to the center of the disc. Only a slight modification of Ranschburg's bradyscope is necessary in order to permit opening in the four cardinal directions, i.e., downward, rightward, upward, and leftward.

In Ranschburg's design, the exposure material placed radially on a cardboard disc is seen through a radial slit in the cover. A double-pawl ratchet wheel electrically operated by magnets revolves the disc which is divided into 60 sectors. The radial aperture in the lid is 1½ in. wide and varies in height from 3/16 in. on the inside to 5/16 in. near the edge. The apparatus is mounted on a hinged base; so the face of the disc can be placed horizontally or supported at an angle ranging from approximately 45 to 60 degrees. The lid may be screwed in position by two screw nuts at the right and left of the circular rim surrounding the inside mechanism. The nuts are on the diagonals passing through the center of the apparatus. The right and left slots at the two sides of the lid are in the form of an inverted L in some makes so that when the lid is put down and slightly turned in the direction of the horizontal slot it can be securely locked to the body of the apparatus.

Now the obvious adaptation for vertical exposure is to cut two similar inverted slots or just two vertical ones at the top and bottom edges of the lid so that the circumference of the cover is divided into four equal arcs. When vertical exposure is desired, the newly cut slots should be used for screwing down the lid to the frame. By this simple modification, the radial aperture on the lid can be in any of the four main positions, namely, right, left, top, or bottom of the center of the machine. There is no permanent alteration to the apparatus that would interfere with its original purpose.

For the purpose of securing intermediate positions of the exposure aperture in any of the four quadrants, the side screw nuts can be done away with and the whole circumference of the cover divided into degrees. A pointer is fastened to the rim of the apparatus so that the cover may be placed in any desired position with the aperture set at any number of degrees from the vertical or from the horizontal.

In summary, the special feature of the quadrant tachistoscope is that it can be used for both vertical and horizontal exposure. All forms of exposure apparatus can be easily adapted for vertical exposure by some simple modification. Four minor changes in the Ranschburg exposure apparatus or bradyscope are suggested for rendering it capable of exposure in any direction: (1) the whole apparatus may be turned around; (2) three more apertures may be cut on the cover; (3) two similar slots may be cut on the opposite edges of the cover; and (4) the whole circumference of the cover may be divided into degrees so that it may be turned in any desired position other than the four main positions. The third alteration serves the same purpose as the first two which are not practical. The fourth modification renders the apparatus more generally useful since it then gives a variety of positions of the exposure window.

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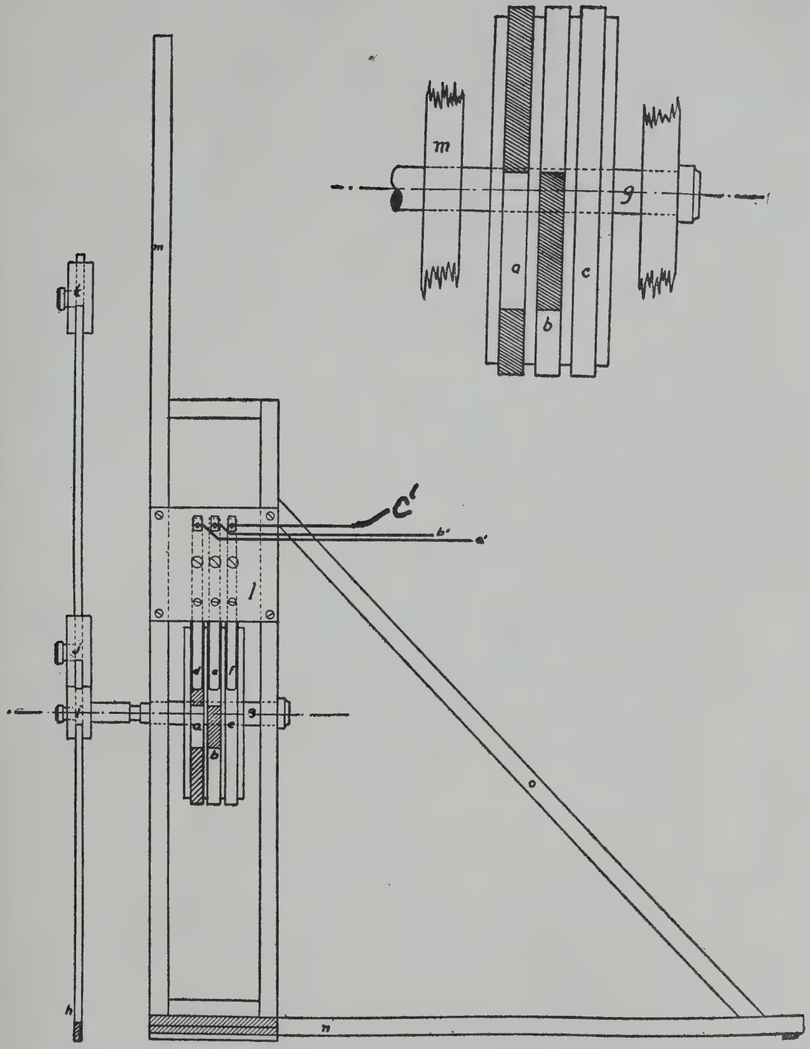
A MODIFIED GRAVITY CHRONOSCOPE

H. R. Crosland

The device for controlling the time of exposures in the Dodge tachistoscope which I described in 1926 (1) proved in later usage to possess three defects. The apparatus now to be described was constructed to surmount difficulties arising from these defects. It is to be recalled that the essential features of the earlier chronoscope were two: first, a large wooden disc rotating on an axle constructed through its center; and, second, copper contact-plates mounted on the face of the disc to engage contact-brushes during the rotation, the latter being afforded by the falling of a weight attached to lever-arms running through the axle parallel with the disc. The lengths of the two plates, on one side, were determined to control a given duration of exposure, such as 150σ, in each of the pre- and the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C'
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FIGURE 1
LATERAL VIEW OF MODIFIED GRAVITY CHRONOSCOPE

main-exposure chambers of the tachistoscope. On the reverse side of the disc, a continuous plate was at all times engaged by a contact-brush. This was by wiring connected through the disc with each of the other contact-plates, to complete one circuit or the other. At the time of its construction, this apparatus functioned well, judged by the tuning-fork, smoked-paper records of check which were obtained. But intervening years, lack of use, and many excursions around the laboratory may have exacted their toll: the apparatus did not "click" when needed again in 1931.

Its three defects were ascertained to be as follows: (1) An inability to maintain an equal and constant pressure of all contact-brushes against the sides of the disc. In other words, the tension of the brushes and the swaying motions of the disc caused variable records of time. (2) The warping of the 3-ply, veneer wood, of which the disc was constructed. This fact broke the contacts of the brushes many times and increased the wobbly motion of the disc as it rotated, thus adding to the variations which were uncontrollable. And (3), at its best, the apparatus was not easily calibrated through the carbon paper, tuning-fork method. The frequency of the above-mentioned variations made this checking procedure unendurable. Hence, measures were instituted[1] to revamp the device. The apparatus which resulted, quite different in one important principle, is now to be pictured and described.

Figures 1 and 2 show respectively the lateral and front views of the new chronoscope.

Employing the wooden base and upright (*p, n, o, m* of Figures 1 and 2) from the older apparatus, we caused to be constructed around the old axle a wooden wheel, much smaller and very much wider than the ancient disc, held tightly around the axle by set-screws. This wooden wheel is shown in Figure 1 by the letters *t, b, c,* and *g*. In fact, *a, b,* and *c* are sectors in bands or ridges surrounding the wheel. A sector of copper, represented by unshaded *a,* interrupts a band of non-conducting leather circumscribing the wheel. Conversely, *b,* shaded, represents a sector of leather which interrupts a continuous band of copper, to break the circuit of the latter. *a* and *b* are equal in size and exactly parallel. *c* is an unbroken band of copper completely encircling the wheel. The three sectors of copper, *a,* that interrupted by *b,* and *c,* are interconnected, thus to complete the circuit from *c* to either *b*'s copper band or *a.* Just above the three copper bands (*a, b, c*) one will see *d, e,* and *f,* respectively. They are brass spring contacts, each carrying a tungsten point, riding with constant pressure on the bands just beneath them. The tensions of them are maintained and controlled by screws which run through them (see Figure 2). They connect with *a*′,

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[1] Most of the credit for the principle of this apparatus as it now stands, and for the fine workmanship, is due Mr. C. N. Crocker, expert mechanician, of Hendershott's Gun Store, Eugene, Oregon.

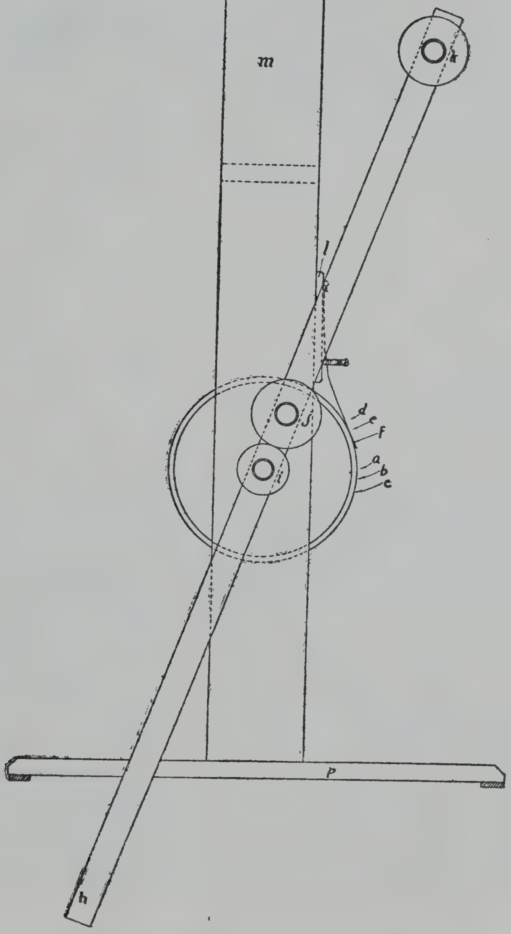
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FIGURE 2
Front View of Modified Gravity Chronoscope

b′, and *c′*, respectively, parts of wire lead No. 1. Sector or band *c* is continuously in circuit, directly by lead No. 1, with the wall socket of the A.C. supply; it is indirectly (through *b*'s band and *a*) in circuit with the pre- and main-exposure fields. The copper continuation of *b* is directly in circuit with the pre-exposure field and, indirectly through *c*, with the wall socket. Sector *a* is directly in circuit with the main-exposure field and, indirectly, through *c*, with the wall socket. Lead No. 2 goes directly from the wall socket to both the pre- and main-exposure fields.

When a sliding peg-lever (not here shown) is disengaged from contact with lever-arm *h*, the weight *k* is allowed to fall. Its fall causes *i-g* to rotate. As long as the copper band of *b* is engaged by *e* the pre-exposure field is illuminated. When leather sector *b* is engaged by *e*, this light is extinguished and the main-exposure field is illuminated through the contacting of *a* by *d*. (In recent experiments the main-exposure light has been controlled to endure exactly 100σ, the accuracy of which exposure-time we have checked through the employment of a 60-cycle Cenco impulse-counter. This, in an adjoining room, has been wired in parallel with the lamp, just between it and the timer.) The continuation and completion of the fall of *k* closes the circuit of *a*, thus extinguishing the main light and renewing the pre-exposure illumination. A switch has, however, been inserted between the copper bands and the tachistoscope lights, in lead No. 1, to discontinue the pre-exposure illumination until a moment of time just prior to an exposure proper.

This apparatus is, in all other respects, very similar to that of 1926. The form of its present construction was intended to remove the variations of exposure time, to hold exposures constant. It has no disc to sway or to warp; and the movements of the axle, from side to side, incident to swaying motions of lever-arms and weights, do not appreciably affect the contacts to break them or to modify them otherwise by changes in pressure. Apparently our purpose has been fully achieved, for in more than 5000 exposures[2] in the course of two years, each checked against the readings of a Cenco 60-cycle impulse-counter, which measured the duration of the electric current at the light bulb itself, not a single variation has been discovered. Without exception the impulse-counter has recorded 12 units, that is, 1/10 of a second or 100σ. Our timer is, therefore, as accurate as the Cenco clock is. The latter may itself vary by as much as from 0 to 2 phases or from 0 to 1/60 of a second. The municipal source of current employs a master clock accurate to 1/60 of a second. Hence, the above-described chronoscope does not vary at all, except as the A.C. supply and the Cenco counter themselves vary in very narrow limits.

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[2] The results from the experiments in which these exposures were obtained are to be published later.

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A DEVICE FOR NUMERICALLY RECORDING EITHER ROTARY OR LINEAR MOVEMENTS OF AN OSCILLATORY CHARACTER

O. H. MOWRER[1]

During the course of an experimental study of the function of the non-acoustic labyrinth in birds, it became evident that a wooden perch of suitable size, supported at the ends by light metal bearings, would afford a convenient and highly useful device for testing the effect of various labyrinthine operations upon bodily equilibrium (as well as for investigating certain other problems), providing that some suitable method could be developed for obtaining a precise quantitative record of the rotary oscillations of the perch produced by the bird in attempting to maintain its balance. With this end in view, numerous electrical and mechanical arrangements of conventional design were considered but were successively discarded because of various disadvantages which precluded their satisfactory use.

The problem was finally successfully solved by means of the apparatus

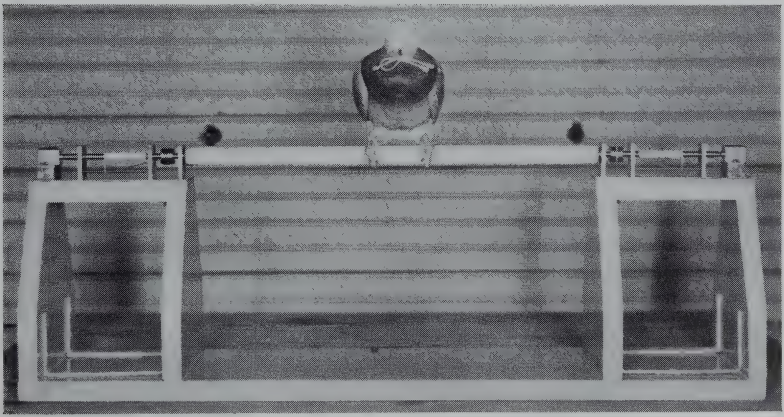

FIGURE 1

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[1] The writer is a National Research Fellow in the Biological Sciences.

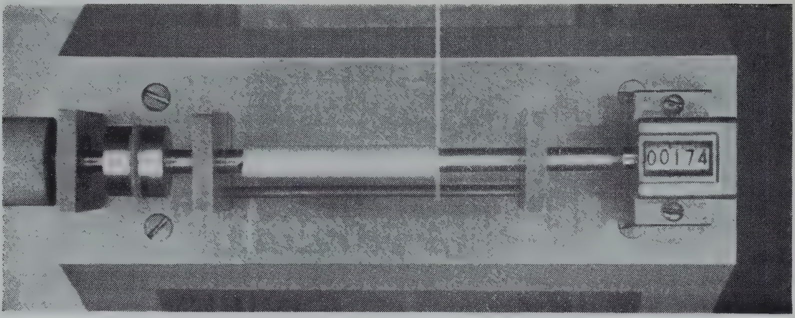

FIGURE 2

pictured in Figure 1. Reference to the close-up shown in Figure 2 will afford a clearer notion of the mechanical principles involved. The string which is wrapped around the horizontal steel shaft in the clockwise direction (regarded from the counter end of the shaft) goes down and around the small brass rod (which acts as a kind of stabilizer) and thence, by way of a suitable system of pulleys, to a small weight. This arrangement causes a constant torque to be exerted on the steel shaft in the counterclockwise direction. A small spiral spring, counter-sunk in the end of the shaft toward the counter, keeps the chamois-skin clutch on the opposite end of the shaft in positive contact with the polished metal disk attached to the axle which supports the right end of the perch. By means of a narrow horizontal slit in the right end of the steel shaft and a tiny key-pin projecting out through this slit from the somewhat smaller shaft of the counter, it is possible to secure perfect transmission of all rotary movement of the main shaft to the counter without rigidly articulating the two and thereby eliminating the end-play in the steel shaft, so essential for maintaining an absolutely uniform yet relatively slight pressure on the face of the clutch. Thus, with proper tension on the spring in the end of the shaft and with a weight of appropriate mass on the end of the string, the clutch holds and therefore causes the counter to record whenever the perch rotates in the counterclockwise direction but slips and therefore does not affect the counter whenever the perch rotates in the clockwise direction. A slightly modified duplication of this arrangement at the other end of the perch makes it possible likewise to record the rotation of the perch in the opposite, i.e., clockwise, direction.

When the proper adjustment of this device has once been obtained, its performance is quite stable and highly accurate. Tests which have been made indicate that it is possible to record even fairly rapid oscillations of the perch with an error consistently less than 1 per cent. Furthermore, the apparatus is practically noiseless, works smoothly, and requires a relatively small amount of energy to actuate it. In the apparatus shown in the illustrations, a weight of 15 grams attached to a string wound around the perch (2 cm. in diameter) is sufficient to produce rotation of the perch and registration on the appropriate counter; and it is highly probable that, if desired, an instrument of similar design could be constructed which would operate with even much greater ease. (Since the pictures shown in the illustrations were taken, a "braking" arrangement has been added which makes its possible to increase the mechanical resistance of the perch any desired amount and to lock it in position when a record is not being made.) The only disadvantage which this apparatus has (for the type of work for which it was designed) is that, as the counters register, the strings wind off of the horizontal shafts and therefore have to be rewound from time to time. However, if this should prove a serious inconvenience, the task of rewinding the strings could be greatly facilitated by the use of a simple crank and disengaging gear arrangement.

In the counters used in the device here described, the extreme right-hand digit represents tenths of revolutions, i.e., angular units of 36 degrees. The other four digits represent complete revolutions. For the purposes for which this apparatus was constructed, this type of counter is entirely satisfactory; however, in the event that still more refined registration were desired, it could easily be obtained in degrees, or even in minutes or seconds, by means of a suitable dial and pointer combination.

In order to record oscillatory movements of a linear character, all that is necessary is merely to replace the perch by a grooved wheel of appropriate dimensions, over which a thread or string is run in the one direction to the subject whose movements are to be recorded and in the other direction to a small weight. (A second string fastened to the subject and running in the direction opposite to that of the first string and over a pulley to another weight of appropriate mass would, of course, cancel any influence which the first weight might tend to have upon the subject's movements.)

It is probably unnecessary to attempt to indicate all of the experimental situations in which a device of the type described above might be profitably used. The peculiar advantages of this kind of apparatus for numerically recording both rotary and linear oscillations have already been suggested. In situations in which graphic records are for some reason essential, a device such as described in this paper might profitably be used in conjunction with the graphically recording apparatus, thereby averting the task of subsequently translating the graphic records thus obtained into numerical terms, a procedure which is usually necessary where accurate, quantitative comparisons are required and which, for its most satisfactory accomplishment, demands the use of special apparatus and an exceedingly great amount of time. There are, of course, other possible uses for the device besides those indicated above, but they are of such a highly specialized nature as to make a description of them seem unwarranted at this time.

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A METHOD FOR OBTAINING SMALL SAMPLES OF SALIVA

Harold V. Gaskill

In a study concerned with the objective measurement of emotional reactions, the hydrogen ion concentration of saliva samples obtained at intervals was used as one variable. Quite adequate means were developed for determining accurately the hydrogen ion concentration of very small samples of saliva. The greatest difficulty encountered was that of obtaining and preserving the sample.

Methods previously used were rejected for various reasons. The drainage tube as described by Lashley (2) alters the pH of saliva simply because it is a foreign object in the mouth.[1] Obtaining the sample by the use of a platinum wire loop, or similar instrument, as described by Starr (3) and others was thought to be inefficient because of the prolonged contact of the sample with the air.[2]

Ordinary hypodermic syringes were used for obtaining samples of saliva. The contact of the saliva with air was very greatly minimized, and the introduction of a foreign object into the mouth was momentary. A 5-cubic-centimeter syringe made of Jena glass (analogous to pyrex) equipped with a 20-gauge silver needle 2 inches in length was used. The sharp points on the needles were ground off and rounded to bluntness. Thus with no discomfort to the subject the blunt needle could be passed between his lips, without causing him even to part his lips, or to turn his head. By inserting the needle always on the left side of the mouth, between the upper teeth and the buccal membrane the sample of saliva could be taken from the same position, relatively, each time. The position was made relatively constant for all subjects by placing the end of the needle opposite the second praemolar at its point of emergence from the gum. Preliminary trials made the obtaining of the saliva sample a routine matter, and minimized the distraction. In studies in which a rather continuous record of pH changes is desired, samples will necessarily be taken frequently. This frequency will make for quicker adaptation and for minimizing the distraction.[3]

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[1] A clean pebble held between the teeth changed the pH in successive samples by one-tenth as the minimum change.

[2] Samples taken by this method were higher in alkalinity than those obtained almost simultaneously by our method. Hence, the conclusion.

No chemical agents were used in cleaning or in sterilizing syringes and needles. They were boiled for 5 minutes in triple distilled water through which air passed over soda lime had been drawn. Syringes and needles were stored in large glass-stoppered bottles. Before each syringe-and-needle combination was used it was rinsed by drawing in and expelling some of this treated distilled water. This rinsing was also repeated before obtaining each sample of saliva. One syringe and needle was used for six samples and then replaced by another combination from the sterilized supply.

The efficacy of cleaning and rinsing syringes and needles as described above was determined by taking a large sample of saliva, allowing it to remain in the syringe, and measuring the pH of separate small portions of this sample. This clearly indicated that the hydrogen ion concentration of the sample was not altered after it was drawn into the syringe. The method of cleaning by rinsing was also shown to have no effect on subsequent samples—and to be an efficient means of removing all traces (in so far as pH measurements are concerned) of the previous sample. The efficacy of the method also is due to the fact that no chemical agents were used in sterilizing or rinsing syringes and needles. Samples of known pH were checked in the same way, and found to be unaltered.

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[3] In our study (cf.1) samples were obtained at 2-minute intervals. All of the subjects reported that after the first two pre-experimental periods (each of 45 minutes' duration) the insertion of the syringe offered no distraction.

A METHOD FOR PRESERVING SMALL SAMPLES OF SALIVA FOR SUBSEQUENT pH DETERMINATION[1]

Harold V. Gaskill

In the use of the hydrogen ion concentration of samples of saliva as one variable in a study utilizing physiological registration the investigator faces three problems, (1) a method of potentiometric titration adaptable to very small samples, e.g., two or three drops, (2) a method of preserving those samples unchanged between the time at which they are taken and the time at which pH measurements are made, and (3) a suitable electrode vessel.

The first of these problems was adequately met by the use of a modification of Biilman's (2) quinhydrone electrode.[2] Very small samples must be used, of course, when a large number of samples are taken in a relatively short time. This is especially true if the investigator is interested in small changes or rapid changes, and wishes to approximate, as nearly as possible, a continuous record of changes in salivary pH.

The second problem—preserving the sample—was met by constructing small pyrex bottles and fitting them with air-tight caps. Several types and sizes were given preliminary trial. Pyrex glass was necessary, of course, to avoid contamination of the sample which occurs when silicate glass is used. The bottle finally selected was slightly under 5-16 inch in inside diameter, 1 inch in height, and made with a round bottom. Rubber caps, sold as "Rubber Policemen," were quite satisfactory as covers. Ground-glass stoppers fitted in the sample bottles were tried, but the pH of samples stored in bottles equipped with rubber caps remained unchanged for a period of 48 hours—as did those in the glass-stoppered bottles. The rubber caps were given preference to obviate the necessity of keeping two or three hundred bottle-and-stopper combinations intact.

The third problem—a suitable electrode vessel—was solved by using the sample bottle *as* the electrode vessel. In this way, when pH determinations were made, the cap was slipped off the sample bottle as it was put up around the platinum wire electrode and agar bridge (see Figure 1). Contact of the sample with air was only momentary. The size of the electrode and bridge which dipped into the sample bottle determined its size, as did the facility with which it could be cleaned and made ready for subsequent use.

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[1] The greater portion of this work was done by Professor G. M. McClure of the Soils Department of The Ohio State University.

[2] For complete description of electrode, half-cell, and technique of titration cf. (2).

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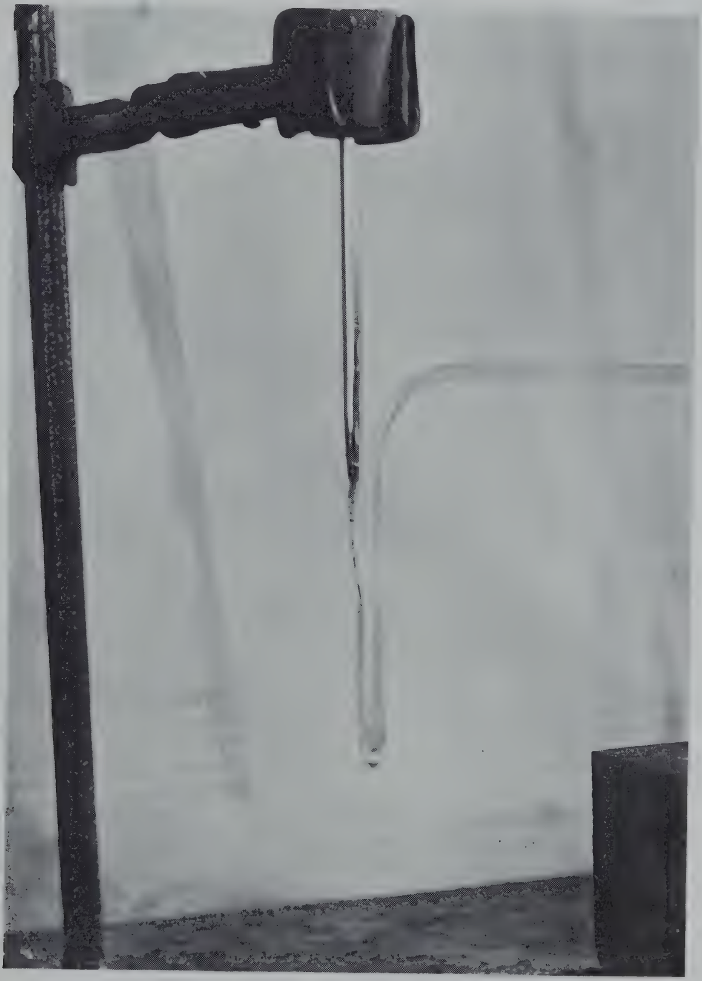

FIGURE 1

BOOKS

KNIGHT DUNLAP. *Habits: Their Making and Unmaking.* New York: Liveright, 1932. Pp. x+326.

The years since Ebbinghaus' demonstration that memorial functions were amenable to fruitful experimental measurement have brought a mounting tide of publications in the general field of learning and memory. Frequency of publication has not brought knowledge in direct proportion, but as a result of the work which has been done he who would experiment in the present year has available a considerable body of solid fact upon which to build and a fairly secure methodology for a wide range of problems. The research upon learning has, however, taught us much less than it might have done. There are several reasons why this is so; two will be mentioned here because of the fact that Professor Dunlap's book has an important relation to them.

1. Experimenters have often planned their experiments with little apparent effort or intent to coordinate their results with already published work. Research which might, otherwise, have advanced greatly our knowledge of a problem has failed to advance it or has added only a little, because of unnecessary innovations in technique or because of flat disregard of prior work. We have had historical introductions in abundance, but of continuity there has often been less than enough to support any important amount of generality. Nevertheless, generalizations have been formulated and, although often hasty and insecure, have become the "principles" of the textbooks and the unquestioned foundation for educational application.

2. In part, perhaps, because of the lack of consistent factual knowledge upon the subject, and partly by virtue of sheer disregard, general interpretations of learning which have attempted to see its constituent problems in a critical, systematic perspective and which have been able to change progressively as research advanced have been few. A sparse handful of the leaders have sought to make research yield generality and to make theory stimulate and interpret fact, although even with some of them systematic perspective has remained implicit, or at least unpublished. In one or two cases theory has hardened into dogma among the followers, while the author of the theory moved on to a modified interpretation as research continued. The persistent use, by the followers, of the vocabulary of theory, often outmoded, has given sometimes a spuriously great impression of theoretical concern where a critical analysis of the concepts actually involved has been slight. If an ordered structure of knowledge about learning is to be built, solid experimental fact and theoretical interpretation need to move forward together.

Professor Dunlap's book is an interpretation of learning which aims to achieve critical and articulate theory in order to understand the ways in which learning goes on in actual human living. This treatment involves neither a survey of experimental work nor a systematic and exhaustive analysis of theory, but as Dunlap's own organization of some of the basic concepts of the field it contributes both to informed experimenting and to critical theorizing. Its primary purpose is an explication of the ways in which the processes of learning and unlearning are interrelated and the application of the conclusions to some of the problems of personality adjustment. Fulfilment of this purpose requires, however, a background of general concepts and the treatment of these concepts pervades the book. Chapters I to VIII are directly concerned with them, and Chapters IX to XII deal with their application to personal and social adjustment, the breaking of specific bad habits, habits of emotional response, and learning ability and intelligence.

Professor Dunlap writes as follows about the conceptual structure of the book: "I suppose that my treatment of the psychology of learning will seem to many readers to be radical, even revolutionary. It will be found that I have discarded the conventional formulations and flouted the 'principles of learning' set forth in widely used texts. These revolutionary features, however, are merely the result of attempting to harmonize and interpret the products of the experimental work of many psychologists; and the revolt is directed only against the theories and the traditions which originated before the experimental era of psychology, and which need to be swept away in order that the experimental results may be understood. My function in this matter is that of an organizer, rather than that of an innovator. I am by no means under the illusion that my interpretations are final" (pp. viii-ix). Whether his treatment of learning will appear radical or not will be a function of the reader's own degree of organization with respect to the field of learning. If viewed from the reference point of conventional chapters on the subject, and of the common glib generalizations, the treatment will seem revolutionary. It is, however, one for which the evidence has been steadily accumulating in the more technical publications for years, and much of the first eight chapters is a bringing together of the implications of a large amount of the more recent work on learning. From the point of view of contemporary research, the revolutionary character of the formulations is less pronounced, but they are none the less important.

The notion that habit is an unvarying and stereotyped response is shown at the outset to be invalid. "In fact, it is safe to say that any habit whatever is really a tendency to a response which varies constantly within certain limits, and the 'invariability' of a habit is determined by the limits of measurement employed in describing the habit" (p. 9). Moreover, "the process through which we learn is not, in general, the process we learn"

(p. 33). The difference between the two is variable, with zero as a limit. The moment this lack of identity between learning and learned responses is clearly recognized, the usual frequency theory loses any meaning. Repetition is important, but it is repetition of performance, of trial rather than of success. "The response to be learned, in short, is an ideal, which is present in the learning, and which is actualized in the result" (p. 36). The importance of "thinking" and of its related functions, implied in this sentence, is dealt with in some detail. This treatment is particularly valuable in view of the current tendency to disregard ideational factors in learning.

The discussion of physiological theories of learning offers a refreshing antidote to the inveterate proclivity of psychologists to make theories of neural function bear the weight of explanation of psychological events. "For the present," writes Dunlap, "the only available theory of brain action in learning is that the brain does act in learning; but that the nature of its action is unknown" (p. 58). As a part of the same problem, Pavlov's "physiological" examination of the function of the nervous system by means of the conditioned reflex is considered in a manner equally refreshing. Instead of upon the nervous system, Pavlov appears to have been working upon "problems of animal learning of the classic psychological type, in which it is admitted that the brain plays a vital, but mysterious, role" (p. 70), and his investigations have "revealed no modification of response which is not of a familiar type" (p. 62). The conditioned response is, moreover, as the succeeding chapter shows, "among the most complicated of learning processes" (p. 85), instead of being the simple prototype which some enthusiasts have claimed.

The fifth chapter, on "The Process of Learning" is the most technical and the most clearly designed for students of learning. It opens with a more detailed exposition of the principle, already mentioned, that in practice the same response may be repeated with increasing probability of recurrence, or in a succession of responses the occurrence of each may make its recurrence less probable. The *alpha, beta,* and *gamma* hypotheses, enunciated in earlier publications (1, 2) are made the organizing focus for a discussion of how learning proceeds and of types of learning. The *alpha* hypothesis, that occurrence of a response increases the probability of is recurrence upon presentation of the same stimulus pattern, and the *gamma* hypothesis that occurrence has no effect on the probability of recurrence, are said to hold under certain narrowly defined conditions. The *beta* hypothesis, which is the reverse of the *alpha,* applies, however, to a wide range of cases and its implications are examined at some length. In negative practice, where repetition abolishes response, "the determining factors are the thoughts and desires involved in the practice . . ." (p. 96). The chapter, which contains much that cannot be indicated in the present brief compass, is an important analysis of learning. The following one upon "The Conditions of Efficient Learning" considers certain determining factors in a brief summary, one of the interesting features of which is Dunlap's rejection of the "kinaesthetic" theory of learning.

Some of the general problems of retention and memory form the theme of Chapters VII and VIII. Retention is an abstraction which refers to the possibility of reproduction and which may be measured in a variety of ways. The consideration which these ways receive is critical and important. Mere retention is not, however, to be called memory. By memory Dunlap means "the capacity, or else the fact, of remembering" (p. 146), when remembering is "recalling with recognition" (p. 147) and is an ideational process. Conditions which affect memory and forgetting are discussed against the background of the earlier chapters. The treatment of retention and memory, while selective and brief, contains much sound analysis and recognition of the theoretical issues involved, while it offers, at the same time, practical advice.

The next three chapters represent an application of the interpretations which comprise the first part of the book to the problems of social adjustment. One of the major contributions of these chapters lies in their demonstration that personality adjustments are, when those which result directly from organic pathology are omitted, a special branch of the psychology of learning. That this is true is illustrated specifically in Dunlap's discussion of the learning and unlearning of stammering, tics, and sexual maladjustments, a discussion which, as regards both point of view and remedial technique, will be of value to anyone who deals with these and related phenomena. The same may be said of his treatment of habits of emotional response, the chapter upon which contains an examination of the James-Lange theory, and which leads to the conclusion that "in forming 'good' emotional habits and in breaking 'bad' ones, we have to deal primarily with thought and with thought habits" (p. 245).

The interrelations of "Learning Ability and Intelligence," which conclude the volume, should be read by everyone who prosposes to think about the problems of "intelligence" or to "test" it. There are, in his discussion, three intelligences: Intelligence I, which means knowledge or skill; Intelligence II, which designates learning ability; and Intelligence III, which covers "a vague collection of assumed abilities, including the ability to learn" (p. 253). Examination of these varied meanings and of what the intelligence tests measure involves a critical definition of learning ability and leads to the conclusion that "the only tests we have measure only intelligence I directly" (p. 254), and that "an essential condition of adequacy of any intelligence test is that the *knowledge which is measured by the test must be as closely as possible allied to the knowledge for which it is desired, on the basis of the test, to predict ability to learn or acquire*" (p. 265).

The book is neither a detailed nor a systematic survey of the field of learning. It is as if the author had set out to write, interestingly and critically but not comprehensively, about the aspects of learning which most interest him. There are gaps, therefore, and many important problems are unmentioned. Many of those which are mentioned are passed over briefly. Contrary views are left largely unconsidered and the reader is forced to take on faith the interpretations given, for there are no bibliographical references to the experimental and theoretical work upon which the discussion draws and there is no specific treatment of this work as such. The Appendix contains, besides a brief historical note, a bibliography, covering 48 pages, prepared by Dr. Willis C. Beasley. The titles cited are representative, well chosen, and bear directly upon the chapters under which they are classified, but there is no reference to them in the text and the reader must correlate the two for himself.

Professor Dunlap has written, apparently, for both psychologist and nonpsychologist. Both will, in the reviewer's opinion, find the book valuable. The layman will gain from it a convincing picture of a part of what the psychologist is doing under the name of experimentation and theorizing upon learning, and of what such work implies for the problems of human adjustment. His respect for research should be enhanced thereby. The psychologist, although he will see it as a general essay rather than as a comprehensive treatise, will find in it an antidote to loose thinking on many problems and an important and suggestive interpretation of many of the basic concepts of learning, an interpretation of a kind which the subject needs.

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